

REPORT

OF THE

NINETEENTH MEETING

OF THE



BRITISH ASSOCIATION

FOR THE

ADVANCEMENT OF SCIENCE;

HELD AT BIRMINGHAM IN SEPTEMBER 1849.

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OBJECTS AND RULES

OF

THE ASSOCIATION.

OBJECTS.

THE ASSOCIATION contemplates no interference with the ground occupied by other Institutions. Its objects are,—To give a stronger impulse and a more systematic direction to scientific inquiry,—to promote the intercourse of those who cultivate Science in different parts of the British Empire, with one another, and with foreign philosophers,—to obtain a more general attention to the objects of Science, and a removal of any disadvantages of a public kind which impede its progress.

RULES.

ADMISSION OF MEMBERS AND ASSOCIATES.

All Persons who have attended the first Meeting shall be entitled to become Members of the Association, upon subscribing an obligation to conform to its Rules.

The Fellows and Members of Chartered Literary and Philosophical Societies publishing Transactions, in the British Empire, shall be entitled, in like manner, to become Members of the Association.

The Officers and Members of the Councils, or Managing Committees, of Philosophical Institutions, shall be entitled, in like manner, to become Members of the Association.

All Members of a Philosophical Institution recommended by its Council or Managing Committee, shall be entitled, in like manner, to become Members of the Association.

Persons not belonging to such Institutions shall be elected by the General Committee or Council, to become Life Members of the Association, Annual Subscribers, or Associates for the year, subject to the approval of a General Meeting.

COMPOSITIONS, SUBSCRIPTIONS, AND PRIVILEGES.

LIFE MEMBERS shall pay, on admission, the sum of Ten Pounds. They shall receive *gratuitously* the Reports of the Association which may be published after the date of such payment. They are eligible to all the offices of the Association.

ANNUAL SUBSCRIBERS shall pay, on admission, the sum of Two Pounds, and in each following year the sum of One Pound. They shall receive *gratuitously* the Reports of the Association for the year of their admission and for the years in which they continue to pay *without intermission* their Annual Subscription. By omitting to pay this Subscription in any particular year, Members of this class (Annual Subscribers) *lose for that and all future years* the privilege of receiving the volumes of the Association *gratis*: but they may resume their Membership and other privileges at any subsequent Meeting of the Association, paying on each such occasion the sum of One Pound. They are eligible to all the Offices of the Association.

ASSOCIATES for the year shall pay on admission the sum of One Pound. They shall not receive *gratuitously* the Reports of the Association, nor be eligible to serve on Committees, or to hold any office.

The Association consists of the following classes :—

1. Life Members admitted from 1831 to 1845 inclusive, who have paid on admission Five Pounds as a composition.

2. Life Members who in 1846, or in subsequent years, have paid on admission Ten Pounds as a composition.

3. Annual Members admitted from 1831 to 1839 inclusive, subject to the payment of One Pound annually. [May resume their Membership after intermission of Annual Payment.]

4. Annual Members admitted in any year since 1839, subject to the payment of Two Pounds for the first year, and One Pound in each following year. [May resume their Membership after intermission of Annual Payment.]

5. Associates for the year, subject to the payment of One Pound.

6. Corresponding Members nominated by the Council.

And the Members and Associates will be entitled to receive the annual volume of Reports, *gratis*, or to *purchase* it at reduced (or Members') price, according to the following specification, viz. :—

1. *Gratis*.—Old Life Members who have paid Five Pounds as a composition for Annual Payments, and previous to 1845 a further sum of Two Pounds as a Book Subscription, or, since 1845 a further sum of Five Pounds.

New Life Members who have paid Ten Pounds as a composition.

Annual Members who have not intermitted their Annual Subscription.

2. *At reduced or Members' Prices*.—Old Life Members who have paid Five Pounds as a composition for Annual Payments, but no further sum as a Book Subscription.

Annual Members, who have intermitted their Annual Subscription.

Associates for the year. [Privilege confined to the volume for that year only.]

Subscriptions shall be received by the Treasurer or Secretaries.

MEETINGS.

The Association shall meet annually, for one week, or longer. The place of each Meeting shall be appointed by the General Committee at the previous Meeting; and the Arrangements for it shall be entrusted to the Officers of the Association.

GENERAL COMMITTEE.

The General Committee shall sit during the week of the Meeting, or longer, to transact the business of the Association. It shall consist of the following persons :—

1. Presidents and Officers for the present and preceding years, with authors of Reports in the Transactions of the Association.

2. Members who have communicated any Paper to a Philosophical Society, which has been printed in its Transactions, and which relates to such subjects as are taken into consideration at the Sectional Meetings of the Association.

3. Office-bearers for the time being, or Delegates, altogether not exceeding three in number, from any Philosophical Society publishing Transactions.

4. Office-bearers for the time being, or Delegates, not exceeding three, from Philosophical Institutions established in the place of Meeting, or in any place where the Association has formerly met.

5. Foreigners and other individuals whose assistance is desired, and who are specially nominated in writing for the meeting of the year by the President and General Secretaries.

6. The Presidents, Vice-Presidents, and Secretaries of the Sections are *ex officio* members of the General Committee for the time being.

SECTIONAL COMMITTEES.

The General Committee shall appoint, at each Meeting, Committees, consisting severally of the Members most conversant with the several branches of Science, to advise together for the advancement thereof.

The Committees shall report what subjects of investigation they would particularly recommend to be prosecuted during the ensuing year, and brought under consideration at the next Meeting.

The Committees shall recommend Reports on the state and progress of particular Sciences, to be drawn up from time to time by competent persons, for the information of the Annual Meetings.

COMMITTEE OF RECOMMENDATIONS.

The General Committee shall appoint at each Meeting a Committee, which shall receive and consider the Recommendations of the Sectional Committees, and report to the General Committee the measures which they would advise to be adopted for the advancement of Science.

All Recommendations of Grants of Money, Requests for Special Researches, and Reports on Scientific Subjects, shall be submitted to the Committee of Recommendations, and not taken into consideration by the General Committee, unless previously recommended by the Committee of Recommendations.

LOCAL COMMITTEES.

Local Committees shall be formed by the Officers of the Association to assist in making arrangements for the Meetings.

Local Committees shall have the power of adding to their numbers those Members of the Association whose assistance they may desire.

OFFICERS.

A President, two or more Vice-Presidents, one or more Secretaries, and a Treasurer, shall be annually appointed by the General Committee.

COUNCIL.

In the intervals of the Meetings, the affairs of the Association shall be managed by a Council appointed by the General Committee. The Council may also assemble for the despatch of business during the week of the Meeting.

PAPERS AND COMMUNICATIONS.

The Author of any paper or communication shall be at liberty to reserve his right of property therein.

ACCOUNTS.

The Accounts of the Association shall be audited annually, by Auditors appointed by the Meeting.

I. Table showing the Places and Times of Meeting of the British Association, with Presidents, Vice-Presidents, and Local Secretaries, from its Commencement.

PRESIDENTS.		VICE-PRESIDENTS.		LOCAL SECRETARIES.	
The EARL FITZWILLIAM, D.C.L., F.R.S., F.G.S., &c.	York, September 27, 1831.	{ Rev. W. Vernon Harcourt, M.A., F.R.S., F.G.S. }	{ William Gray, jun., F.G.S. }		
The REV. W. BUCKLAND, D.D., F.R.S., F.G.S., &c.	Oxford, June 19, 1832.	{ Sir David Brewster, F.R.S., F.R.S., &c. }	{ Professor Phillips, F.R.S., F.G.S. }		
The REV. ADAM SEDGWICK, M.A., V.P.R.S., V.P.G.S.,	Cambridge, June 29, 1833	{ G. B. Airy, F.R.S., Astronomer Royal, &c. }	{ Rev. Professor Henslow, M.A., F.L.S., F.G.S. }		
Sir T. MANDOUAL BRISBANE, K.C.B., D.C.L., F.R.S.S., L., &c.	Edinburgh, September 8, 1834.	{ John Dalton, D.C.L., F.R.S., &c. }	{ Rev. W. Whewell, F.R.S. }		
The REV. PROVOST LLOYD, LL.D.	Dublin, August 10, 1835.	{ Sir David Brewster, F.R.S., &c. }	{ Professor Forbes, F.R.S., L., &c. }		
The MARQUIS OF LANSDOWNE, D.C.L., F.R.S., &c.	Bristol, August 22, 1836.	{ Viscount Oxmantown, F.R.S., F.R.A.S. }	{ Sir John Robison, Sec. R.S.E. }		
The EARL OF BURLINGTON, F.R.S., F.G.S., Chan. Univ. London..	Liverpool, September 11, 1837.	{ The Marquis of Northampton, F.R.S., &c. }	{ Sir W. R. Hamilton, Astron. Royal of Ireland, &c. }		
The DUKE OF NORTHUMBERLAND, F.R.S., F.G.S., &c.	Newcastle-on-Tyne, August 20, 1838.	{ Rev. W. D. Conybeare, F.R.S., F.G.S. }	{ Rev. Professor Lloyd, F.R.S. }		
The REV. W. VERNON HARCOURT, M.A., F.R.S., &c.	Birmingham, August 26, 1839.	{ J. C. Pritchard, M.D., F.R.S. }	{ Professor Daubeny, M.D., F.R.S., &c. }		
The MARQUIS OF BREADALBANE, F.R.S.	Glasgow, September 17, 1840.	{ The Bishop of Durham, F.R.S., F.S.A. }	{ V. F. Hovenden, Esq. }		
The REV. PROFESSOR WHEWELL, F.R.S., &c.	Plymouth, July 29, 1841.	{ John Dalton, D.C.L., F.R.S. }	{ Professor Traill, M.D. }		
LORD FRANCIS EGERTON, F.G.S.	Manchester, June 23, 1842.	{ Sir Philip de Grey Egerton, Bart., F.R.S., F.G.S. }	{ Wm. Wallace Currie, Esq. }		
The EARL OF ROSSE, F.R.S., &c.	Cork, August 17, 1843.	{ Rev. W. Whewell, F.R.S., &c. }	{ Joseph N. Walker, Pres. Royal Institution, Liverpool. }		
		{ The Rev. W. Vernon Harcourt, F.R.S., &c. }	{ John Adamson, F.L.S., &c. }		
		{ Pridcaux John Selby, Esq., F.R.S.E. }	{ Wm. Hutton, F.G.S. }		
		{ Marquis of Northampton. Earl of Dartmouth. }	{ Professor Johnston, M.A., F.R.S. }		
		{ The Rev. T. R. Robinson, D.D. }	{ George Barker, Esq., F.R.S. }		
		{ John Corrie, Esq., F.R.S. }	{ Peyton Blakiston, M.D. }		
		{ Very Rev. Principal Macfarlane }	{ Joseph Hodgson, Esq., F.R.S. }		
		{ Major-General Lord Greenock, F.R.S.E. }	{ Follett Osler, Esq. }		
		{ Sir David Brewster, F.R.S., &c. }	{ Andrew Liddell, Esq. }		
		{ Sir T. M. Brisbane, Bart., F.R.S. }	{ Rev. J. P. Nicol, LL.D. }		
		{ The Earl of Morley. Lord Eliot, M.P. }	{ John Strang, Esq. }		
		{ Sir C. Lemon, Bart. Sir T. D. Adlam, Bart. }	{ W. Snow Harris, Esq., F.R.S. Col Hamilton Smith, F.L.S }		
		{ John Dalton, D.C.L., F.R.S., &c. }	{ Robert Were Fox, Esq. Richard Taylor, jun., Esq. }		
		{ Hon. and Rev. W. Herbert, F.L.S., &c. }	{ Peter Clare, Esq., F.R.A.S. }		
		{ Rev. A. Sedgwick, M.A., F.R.S. }	{ W. Fleming, M.D. }		
		{ W. C. Henry, M.D., F.R.S. }	{ James Heywood, Esq., F.R.S. }		
		{ Sir Benjamin Heywood, Bart. }			
		{ Earl of Listowel. Viscount Adare }	{ Professor John Stevelly, M.A. }		
		{ Sir W. R. Hamilton, Pres. R.I.A. }	{ Rev. Jos. Carson, F.T.C. Dublin. }		
		{ Rev. T. R. Robinson, D.D. }	{ Wm. Keleher, Esq. Wm. Clear, Esq. }		

The REV. G. PEACOCK, D.D. (Dean of Ely), F.R.S. YORK, September 26, 1844.	<ul style="list-style-type: none"> Earl Fitzwilliam, F.R.S. Viscount Morpeth, F.G.S. The Hon. John Stuart Wortley, M.P. Sir David Brewster, K.H., F.R.S. Michael Faraday, Esq., D.C.L., F.R.S. Rev. W. V. Harcourt, F.R.S. 	<ul style="list-style-type: none"> William Hatfield, Esq., F.G.S. Thomas Meynell, Esq., F.L.S. Rev. W. Scoresby, LL.D., F.R.S. William West, Esq.
SIR JOHN F. W. HERSCHEL, Bart., F.R.S., &c. CAMBRIDGE, June 19, 1845.	<ul style="list-style-type: none"> The Earl of Hardwicke, The Bishop of Norwich. G. B. Airy, Esq., M.A., D.C.L., F.R.S. The Rev. Professor Sedgwick, M.A., F.R.S. 	<ul style="list-style-type: none"> William Hopkins, Esq., M.A., F.R.S. Professor Ansted, M.A., F.R.S.
SIR RODERICK IMPEY MURCHISON, G.C.S., F.R.S. SOUTHAMPTON, September 10, 1846.	<ul style="list-style-type: none"> The Marquis of Winchester The Earl of Yarborough, D.C.L. Lord Ashburton, D.C.L. Viscount Palmerston, M.P. Right Hon. Charles Shaw Lefevre, M.P. Sir George T. Staunton Bart., M.P., D.C.L., F.R.S. The Lord Bishop of Oxford, F.R.S. Prof. Owen, M.D., F.R.S. Prof. Powell, F.R.S. 	<ul style="list-style-type: none"> Henry Clark, M.D. T. H. C. Moody, Esq.
SIR ROBERT HARRY INGLIS, Bart., D.C.L., F.R.S., M.P. for the University of Oxford Oxford, June 23, 1847.	<ul style="list-style-type: none"> The Earl of Rosse, F.R.S. The Lord Bishop of Oxford, F.R.S. The Vice-Chancellor of the University Thomas G. Bucknall Esquire, Esq., D.C.L., M.P. for the University of Oxford Very Rev. The Dean of Westminster, D.D., F.R.S. Professor Daubeny, M.D., F.R.S. The Rev. Professor Powell, M.A., F.R.S. 	<ul style="list-style-type: none"> Rev. Robert Walker, M.A., F.R.S. Henry Wentworth Acland, Esq., B.M.
The MARQUIS OF NORTHAMPTON, Pres. Royal Society, &c. SWANSEA, August 9, 1848.	<ul style="list-style-type: none"> The Marquis of Bute, K.T. Viscount Adare, F.R.S. Sir H. T. De la Beche, F.R.S., Pres. G. S. The Very Rev. the Dean of Llandaf, F.R.S. Lewis W. Dillwyn, Esq., F.R.S. W. R. Grove, Esq., F.R.S. J. H. Vivian, Esq., M.P., F.R.S. The Lord Bishop of St. David's 	<ul style="list-style-type: none"> Matthew Mogridge, Esq. D. Nicol, M.D.
The REV. T. R. ROBINSON, D.D., M.R.I.A., F.R.A.S. BIRMINGHAM, September 12, 1849.	<ul style="list-style-type: none"> The Earl of Harrowby The Lord Wrottesley, F.R.S. Right Hon. Sir Robert Peel, M.P., D.C.L., F.R.S. Charles Darwin, Esq., M.A., F.R.S., Sec. G.S. Professor Faraday, D.C.L., F.R.S. Sir David Brewster, K.H., LL.D., F.R.S. Rev. Professor Willis, M.A., F.R.S. 	<ul style="list-style-type: none"> Captain Tindal, R.N. William Wills, Esq. Bell Fletcher, Esq., M.D. James Chance, Esq.
SIR DAVID BREWSTER, K.H., LL.D., F.R.S. L. & E. EDINBURGH, July 31, 1850.	<ul style="list-style-type: none"> Right Hon. the Lord Provost of Edinburgh The Earl of Caithcart, K.C.B., F.R.S.E. The Earl of Rosebery, K.T., D.C.L., F.R.S. Right Hon. David Boyle (Lord Justice General), F.R.S.E. General Sir Thomas M. Brisbane, Bart., K.C.B., G.C.H., D.C.L., F.R.S., Pres. R.S.E. Very Rev. John Lee, D.D., V.P.R.S.E., Principal of the University of Edinburgh. Professor W. P. Alison, M.D., V.P.R.S.E. Professor J. D. Forbes, F.R.S., Sec. R.S.E. 	<ul style="list-style-type: none"> Rev. Professor Kelland, M.A., F.R.S. L. & E. Professor Balfour, M.D., F.R.S.E., F.L.S. James Tod, Esq., F.R.S.

II. Table showing the Names of Members of the British Association who have served on the Council in former years.

Acland, Sir Thomas D., Bart., M.P., F.R.S.	Drinkwater, J. E., Esq.
Acland, Professor H. W., B.M., F.R.S.	Durham, Edward Maltby, D.D., Lord Bishop of, F.R.S.
Adamson, John, Esq., F.L.S.	Egerton, Sir Philip de M. Grey, Bart., F.R.S.
Adare, Edwin, Viscount, M.P., F.R.S.	Eliot, Lord, M.P.
Airy, G. B., D.C.L., F.R.S., Astronomer Royal.	Ellesmere, Francis, Earl of, F.G.S.
Ainslie, Rev. Gilbert, D.D., Master of Pembroke Hall, Cambridge.	Estcourt, T. G. B., D.C.L.
Ansted, Professor D. T., M.A., F.R.S.	Faraday, Professor, D.C.L., F.R.S.
Arnott, Neil, M.D., F.R.S.	Fitzwilliam, Charles William, Earl, D.C.L., F.R.S.
Ashburton, William Bingham, Lord, D.C.L.	Fleming, W., M.D.
Babbage, Charles, Esq., F.R.S.	Fletcher, Bell, M.D.
Babington, C. C., Esq., F.L.S.	Forbes, Charles, Esq.
Baily, Francis, Esq., F.R.S.	Forbes, Professor Edward, F.R.S.
Barker, George, Esq., F.R.S.	Forbes, Professor J. D., F.R.S.
Bengough, George, Esq.	Fox, Robert Were, Esq., F.R.S.
Bentham, George, Esq., F.L.S.	Gilbert, Davies, D.C.L., F.R.S.
Bigge, Charles, Esq.	Graham, Professor Thomas, M.A., F.R.S.
Blakiston, Peyton, M.D., F.R.S.	Gray, John E., Esq., F.R.S.
Brewster, Sir David, K.H., LL.D., F.R.S.	Gray, Jonathan, Esq.
Breadalbane, John, Marquis of, K.T., F.R.S.	Gray, William, jun., Esq., F.G.S.
Brisbane, Lieut.-General Sir Thomas M., Bart., K.C.B., G.C.H., D.C.L., F.R.S.	Green, Professor Joseph Henry, F.R.S.
Brown, Robert, D.C.L., F.R.S., President of the Linnean Society.	Greenough, G. B., Esq., F.R.S.
Brunel, Sir M. I., F.R.S.	Grove, W. R., Esq., F.R.S.
Buckland, Very Rev. William, D.D., Dean of Westminster, F.R.S.	Hallam, Henry, Esq., M.A., F.R.S.
Burlington, William, Earl of, M.A., F.R.S., Chancellor of the University of London.	Hamilton, W. J., Esq., Sec.G.S.
Bute, John, Marquis of, K.T.	Hamilton, Sir William R., Astronomer Royal of Ireland, M.R.I.A.
Carlisle, George William Frederick, Earl of, F.G.S.	Harcourt, Rev. William Vernon, M.A., F.R.S.
Carson, Rev. Joseph.	Hardwicke, Charles Philip, Earl of, F.R.S.
Cathcart, Lieut.-General, Earl of, K.C.B., F.R.S.E.	Harford, J. S., D.C.L., F.R.S.
Chalmers, Rev. T., D.D., late Professor of Divinity, Edinburgh.	Harris, Sir W. Snow, F.R.S.
Chance, James, Esq.	Harrowby, The Earl of.
Chester, John Graham, D.D., Lord Bishop of.	Harfeild, William, Esq., F.G.S.
Christie, Professor S. H., M.A., Sec. R.S.	Henslow, Rev. Professor, M.A., F.L.S.
Clare, Peter, Esq., F.R.A.S.	Henry, W. C., M.D., F.R.S.
Clark, Rev. Professor, M.D., F.R.S. (Cambridge).	Herbert, Hon. and Very Rev. William, late Dean of Manchester, LL.D., F.L.S.
Clark, Henry, M.D.	Herschel, Sir John F.W., Bart., D.C.L., F.R.S.
Clark, G. T., Esq.	Heywood, Sir Benjamin, Bart., F.R.S.
Clear, William, Esq.	Heywood, James, Esq., M.P., F.R.S.
Clerke, Major Shadwell, K.H., R.E., F.R.S.	Hodgkin, Thomas, M.D.
Clift, William, Esq., F.R.S.	Hodgkinson, Professor Eaton, F.R.S.
Colquhoun, J. C., Esq., M.P.	Hodgson, Joseph, Esq., F.R.S.
Conybeare, Very Rev. W.D., Dean of Llandaff, M.A., F.R.S.	Hooker, Sir William J., LL.D., F.R.S.
Corrie, John, Esq., F.R.S.	Hope, Rev. F. W., M.A., F.R.S.
Currie, William Wallace, Esq.	Hopkins, William, Esq., M.A., F.R.S.
Dalton, John, D.C.L., F.R.S.	Horner, Leonard, Esq., F.R.S., F.G.S.
Daniell, Professor J. F., F.R.S.	Hovenden, V. F., Esq., M.A.
Dartmouth, William, Earl of, D.C.L., F.R.S.	Hutton, Robert, Esq., F.G.S.
Darwin, Charles, Esq., F.R.S.	Hutton, William, Esq., F.G.S.
Daubeney, Professor Charles G. B., M.D., F.R.S.	Ibbetson, Capt. L. L. Boscawen, K.R.E., F.G.S.
De la Beche, Sir Henry T., F.R.S., Director-General of the Geological Survey of the United Kingdom.	Inglis, Sir Robert H., Bart., D.C.L., M.P., F.R.S.
Dillwyn, Lewis W., Esq., F.R.S.	Jameson, Professor R., F.R.S.
	Jenyns, Rev. Leonard, F.L.S.
	Jerrard, H. B., Esq.
	Johnston, Professor J. F. W., M.A., F.R.S.
	Keleher, William, Esq.
	Lardner, Rev. Dr.
	Lee, Robert, M.D., F.R.S.
	Lansdowne, Henry, Marquis of, D.C.L., F.R.S.
	Latham, R. G., M.D., F.R.S.

- Lefevre, Right Hon. Charles Shaw, Speaker
 of the House of Commons.
 Lemon, Sir Charles, Bart., M.P., F.R.S.
 Liddell, Andrew, Esq.
 Lindley, Professor, Ph.D., F.R.S.
 Listowel, The Earl of.
 Lloyd, Rev. Bartholomew, D.D., late Provost
 of Trinity College, Dublin.
 Lloyd, Rev. Professor, D.D., Provost of
 Trinity College, Dublin, F.R.S.
 Lubbock, Sir John W., Bart., M.A., F.R.S.
 Luby, Rev. Thomas.
 Lyell, Sir Charles, M.A., F.R.S.
 MacCullagh, Professor, D.C.L., M.R.I.A.
 Macfarlane, The Very Rev. Principal.
 MacLeay, William Sharp, Esq., F.L.S.
 MacNeill, Professor Sir John, F.R.S.
 Meynell, Thomas, Jun., Esq., F.L.S.
 Miller, Professor W. H., M.A., F.R.S.
 Moillet, J. L., Esq.
 Moggridge, Matthew, Esq.
 Moody, T. H. C., Esq.
 Moody, T. F., Esq.
 Morley, The Earl of.
 Moseley, Rev. Henry, M.A., F.R.S.
 Mount-Edgcombe, Ernest Augustus, Earl of.
 Murchison, Sir Roderick I., G.C.S., F.R.S.
 Neill, Patrick, M.D., F.R.S.E.
 Nicol, D., M.D.
 Nicol, Rev. J. P., LL.D.
 Northumberland, Hugh, Duke of, K.G., M.A.,
 F.R.S.
 Northampton, Spencer Joshua Alwyne, Mar-
 quis of, V.P.R.S.
 Norwich, Edward Stanley, D.D., F.R.S., late
 Lord Bishop of.
 Ormerod, G. W., Esq., F.G.S.
 Orpen, Thomas Herbert, M.D.
 Owen, Professor Richard, M.D., F.R.S.
 Oxford, Samuel Wilberforce, D.D., Lord
 Bishop of, F.R.S., F.G.S.
 Osler, Follett, Esq.
 Palmerston, Viscount, G.C.B., M.P.
 Peacock, Very Rev. George, D.D., Dean of
 Ely, F.R.S.
 Peel, Rt. Hon. Sir Robert, Bart., M.P.,
 D.C.L., F.R.S.
 Pendarves, E., Esq., F.R.S.
 Phillips, Professor John, F.R.S.
 Porter, G. R., Esq.
 Powell, Rev. Professor, M.A., F.R.S.
 Prichard, J. C., M.D., F.R.S.
 Ramsay, Professor W., M.A.
 Rennie, George, Esq., V.P. & Treas. R.S.
 Rennie, Sir John, F.R.S.
 Richardson, Sir John, M.D., F.R.S.
 Ritchie, Rev. Professor, LL.D., F.R.S.
 Robinson, Rev. J., D.D.
 Robinson, Rev. T. R., D.D., M.R.I.A.
 Robison, Sir John, late Sec. R.S. Edin.
 Roche, James, Esq.
 Roget, Peter Mark, M.D., F.R.S.
 Ross, Capt. Sir James C., R.N., F.R.S.
 Rosse, William, Earl of, M.R.I.A., President
 of the Royal Society.
 Royle, Professor John F., M.D., F.R.S.
 Russell, James, Esq.
 Sabine, Lieut.-Colonel Edward, R.A., For.
 Sec. R.S.
 Sanders, William, Esq., F.G.S.
 Sandon, Lord.
 Scoresby, Rev. W., D.D., F.R.S.
 Sedgwick, Rev. Professor, M.A., F.R.S.
 Selby, Prideaux John, Esq., F.R.S.E.
 Smith, Lieut.-Colonel C. Hamilton, F.R.S.
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 F.R.S.
 St. David's, Connop Thirlwall, D.D., Lord
 Bishop of.
 Stevelly, Professor John, LL.D.
 Strang, John, Esq.
 Strickland, H. E., Esq., F.G.S.
 Sykes, Lieut.-Colonel W. H., F.R.S.
 Symons, B. P., D.D., late Vice-Chancellor of
 the University of Oxford.
 Talbot, W. H. Fox, Esq., M.A., F.R.S.
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 Taylor, John, Esq., F.R.S.
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 Thompson, William, Esq., F.L.S.
 Tindal, Captain, R.N.
 Traill, J. S., M.D.
 Turner, Edward, M.D., F.R.S.
 Turner, Samuel, Esq., F.R.S., F.G.S.
 Turner, Rev. W.
 Vigors, N. A., D.C.L., F.L.S.
 Vivian, J. H., M.P., F.R.S.
 Walker, James, Esq., F.R.S.
 Walker, Joseph N., Esq., F.G.S.
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 Whewell, Rev. William, D.D., F.R.S., Master
 of Trinity College, Cambridge.
 Williams, Professor Charles J. B., M.D., F.R.S.
 Willis, Rev. Professor, M.A., F.R.S.
 Wills, William.
 Winchester, John, Marquis of.
 Woolcombe, Henry, Esq., F.S.A.
 Wrottesley, John, Lord, M.A., F.R.S.
 Yarrell, William, Esq., F.L.S.
 Yarborough, The Earl of, D.C.L.
 Yates, James, Esq., M.A., F.R.S.

BRITISH ASSOCIATION FOR THE

THE GENERAL TREASURER'S ACCOUNT from 8th of August

RECEIPTS.

	£	s.	d.	£	s.	d.
Life Compositions at Swansea				30	0	0
Annual Subscriptions at Swansea and since				150	0	0
Associates' at Swansea				376	0	0
Ladies' Tickets at Swansea				197	0	0
Book Compositions				6	0	0
Dividends on Stock				116	10	1
Sale of Stock (£1000 three per cent. Consols)				917	9	2
From Sale of Publications:—						
Of the 2nd volume			7		2	
3rd "	1	8	0			
4th "	1	4	4			
5th "		15	2			
6th "	2	2	11			
7th "	2	6	0			
8th "	3	0	4			
9th "	1	18	0			
10th "	3	7	6			
11th "	1	19	11			
12th "	3	0	9			
13th "	5	12	0			
14th "	5	4	0			
15th "	15	10	0			
16th "	80	0	0			
17th "	0	6	0			
British Association's Catalogue of Stars	28	1	10			
Lalande's Catalogue of Stars	9	8	2			
Lacaille's Catalogue of Stars	1	16	5			
Lithograph Signatures	0	15	0			
					168	3
						6
					£1961	2
						9

JAMES HEYWOOD, }
 G. R. PORTER, }
 J. W. GILBART, } *Auditors.*

1848 (at Swansea) to 12th of September 1849 (at Birmingham).

[illegible]

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M. Arago, Paris.	M. Kupffer, St. Petersburg.
Dr. A. D. Bache, Philadelphia.	Dr. Langberg, Christiania.
Professor H. von Boguslawski, Breslau.	M. Leverrier, Paris.
Monsieur Boutigny (d'Evreux), Paris.	Baron de Selys-Longchamps, Liège.
Professor Braschmann, Moscow.	Dr. Lamont, Munich.
Chevalier Bunsen.	Baron von Liebig, Giessen.
Charles Buonaparte, Prince of Canino.	Professor Link, Berlin.
M. De la Rive, Geneva.	Professor Matteucci, Pisa.
Professor Dove, Berlin.	Professor von Middendorff, St. Petersburg.
Professor Dumas, Paris.	Professor Nilsson, Sweden.
Dr. J. Milne-Edwards, Paris.	Dr. Ørsted, Copenhagen.
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Professor Encke, Berlin.	Professor Plücker, Bonn.
Dr. A. Erman, Berlin.	Professor C. Ritter, Berlin.
Professor Esmark, Christiania.	Professor H. D. Rogers, Philadelphia.
Professor G. Forchhammer, Copenhagen.	Professor H. Rose, Berlin.
M. Frisiani, Milan.	Professor Schumacher, Altona.
Professor Henry, Princeton, United States.	Baron Senftenberg, Bohemia.
Baron Alexander von Humboldt, Berlin.	Dr. Siljeström, Stockholm.
M. Jacobi, St. Petersburg.	M. Struvè of St. Petersburg.
Professor Jacobi, Königsberg.	Dr. Svanberg, Stockholm.
	Dr. Van der Höven, Leyden.
	Baron Sartorius von Waltershausen, Gotha.
	Professor Wartmann, Lausanne.

REPORT OF THE PROCEEDINGS OF THE COUNCIL IN 1848-49, AS PRESENTED TO THE GENERAL COMMITTEE AT BIRMINGHAM, WEDNESDAY, SEPTEMBER 12, 1849.

I. With reference to the subjects referred to the Council by the General Committee assembled at Swansea, the Council have to report—

1st. That they communicated the recommendation of the General Committee, for the continuance of the Magnetical and Meteorological Observatory at Toronto to the 31st of December, 1850, to Lord John Russell, through the President, the Marquis of Northampton. They have the pleasure of stating that the Observatory has been continued.

2nd. Pursuant to the request of the General Committee, the Council have taken into consideration the expediency of inserting in the Rules of the British Association a paragraph to the effect that those gentlemen who have held the office of President of the Association should subsequently be *ex-officio* members of the Council; and the Council now recommend that a paragraph to that effect should be inserted in the Rules of the Association.

3rd. The sum of 100*l.*, placed by the General Committee at the disposal of the Council for the disbursements connected with the Kew Observatory, has sufficed, under Mr. Ronalds's general superintendence, for the maintenance of the Observatory in the past year as a depository for the books and instruments belonging to the Association; and also for the preparation of the self-registering magnetical instruments, on Mr. Ronalds's plan, for the

Toronto Colonial Observatory. Mr. Birt has completed the reduction and discussion of the series of electrical observations made at Kew; and Mr. Ronalds has drawn up a Report describing the modifications and improvements which he has introduced in the self-registering apparatus during the last year. Both these Reports will be read to Section A. preparatory to a consideration of any further recommendation which it may appear desirable to make for the continued maintenance of the Observatory. In connexion with this subject, the Council have great pleasure in announcing to the General Committee, that Her Majesty's Government, on the joint application of the Marquis of Northampton and Sir John Herschel, have granted to Mr. Ronalds a pecuniary recompense of £250 for the invention of his method of constructing self-registering magnetical and meteorological apparatus. It will be recollected by many members of the General Committee that the subject of self-registering instruments was discussed at the meeting of the British Association at Cambridge, in 1845, upon the application for a grant of money from the funds of the Association to enable Mr. Ronalds to complete an apparatus for that purpose at Kew; and that a recommendation was made on that occasion by the Association to Government—which recommendation was concurred in by the President and Council of the Royal Society—of the expediency of encouraging, by specific pecuniary rewards, the improvement of self-recording magnetical and meteorological apparatus.

As the grant to Mr. Ronalds has been made in consequence of that original recommendation and the favourable reply that was returned to it, and as the apparatus itself has been constructed, and its successful operation shown at the Observatory of the Association, of which Mr. Ronalds is the Honorary Superintendent, the Council have deemed it proper to make this formal, and as they are sure acceptable, announcement of the favourable reception which has been given to the application on Mr. Ronalds's behalf; but they are glad, at the same time, to take the opportunity of expressing the satisfaction with which they have learned that the ingenious invention of Mr. Brooke, for similar purposes, has also received a pecuniary recompense from the Government.

II. The Council regret that they are still unable to announce the publication either of Professor Edward Forbes's Researches in the Ægean Sea, or of the Mountjoy Observations, for which purposes grants of public money have been sanctioned by Her Majesty's Government at the recommendation of the British Association.

III. The Council have added the following names to the list of Corresponding Members of the British Association:—

Professor Plücker of Bonn.

Dr. Siljeström of Stockholm.

Prof. H. D. Rogers of Philadelphia.

IV. Prof. Dove, of Berlin, Corresponding Member of the British Association, having offered to supply the Association with as many copies as might be desired of his Maps of the Monthly Isothermal Lines of the Globe, founded upon the Temperature Tables printed in the volume of the Reports of the British Association for 1848, which maps have been partly engraved and partly lithographed at the expense of the Royal Academy of Sciences at Berlin, the Council directed that Prof. Dove should be requested to supply the Association with 500 copies, on the understanding that the Association should pay for the paper and for taking off the impressions; and that the copies thus furnished should be sold, under the direction of the officers, to Members of the Association at cost price, with the translation of a report

from Prof. Dove, explanatory of the Maps and of the more obvious conclusions deduced from them. The Maps have been completed, but from accidental circumstances the packet containing the first 200 copies, prepared for the Association, has not yet been despatched from Berlin, and cannot be expected to reach England until after the Meeting at Birmingham is over; but copies of the Maps and Report will be forwarded immediately they arrive to Members who may be desirous of purchasing them, and who give their names for that purpose in the Reception Room. The cost of each copy will be 5s. for the three Maps.

V. The Council has directed that the following additions should be made to the Regulations, according to which the volumes of the Reports are distributed to the Members:—

1. That Members who have formerly paid £5 as a life composition, and shall at any future time pay an additional sum of £5, shall be entitled to receive (gratis) the volumes of the Transactions which shall be published after the date of such additional payments.

2. That Members shall have the opportunity of purchasing any of the already published volumes of the Association, of which more than 100 copies remain, at half the price at which the volumes were issued to the public.

VI. The Council have great pleasure in submitting to the General Committee the following list of invitations, from which the General Committee will have to select the place of meeting of the Association in 1850.

a. Invitations received at Swansea by the General Committee, and which stood over after the selection of Birmingham, 1849:—

From Ipswich, for 1849; signed by the High Sheriff, the Bishop of Norwich, and eighty gentlemen of the Eastern Counties.

From Bath, for 1850; signed by the Mayor.

From Derby, for 1850.

b. Invitations received since the Swansea Meeting and communicated to the Council:—

From Edinburgh, for 1850; from the Lord Provost, Magistrates and Council; from the Senatus Academicus; and from the Royal Society of Edinburgh.

From Belfast for 1850 or 1851; from the Town Council; the Royal Academical Institution; the Natural History and Philosophical Society; and from the Harbour Commissioners.

From Manchester, for 1852; from the Royal Institution; the Geological Society; the Natural History Society; the School of Design; and the Mechanics' Institution.

From Hull, for an early meeting; from the Literary and Philosophical Institution.

VII. The Council have received and submit to the General Committee the following letter from Lieut.-Col. Sabine:—

"To the President and Council of the British Association.

"GENTLEMEN,—I beg leave to acquaint you that it is my intention, at the Meeting of the Association at Birmingham, to resign into the hands of the General Committee the office of General Secretary, with which I have been honoured, by annual re-election, for ten successive years.

"I have formed this determination, not from any occurrence which has rendered me less willing than heretofore, to undertake the duties and responsibilities of that office, or to make the sacrifice of time, convenience, and of other interests which it requires; nor from a fear that the kindness and indulgence with which my endeavours to discharge its duties have been

regarded by the General Committee, by the Council, and by the Members of the Association generally, is in any danger of being exhausted; but from the opinion which I entertain that, as a general principle, the disadvantages of such offices being held by the same individual for several years outweigh the advantages,—and that in my own particular case it is safer to act on the general principle than to fancy myself an exception to it.

“I am aware that in Societies in which, as in the British Association, the Presidency is held but for a single year, it may be desirable that the next principal executive officer should be more permanent than when the Presidency is held for a longer period. But the office of General Secretary of the British Association for the Advancement of Science is one which confers honour and distinction on the individual who holds it, not only in Britain, but in all countries where science is enjoyed or its advancement desired; and as such it may justly be regarded as an object of reasonable ambition. When the Meeting at Birmingham shall have concluded, I shall have completed a cycle of ten years, and I consider that the time will then be fully arrived when, with propriety as regards myself, and with a due consideration of the interests of others, and pre-eminently those of the Association itself, I may resign the trust with which I have been honoured, into the hands from which I received it.

“I have thought it my duty to give you this early intimation of my intention, as it will probably be considered right that the recommendation of my successor should proceed from the Council.

“I have, &c.,

“EDWARD SABINE.”

RECOMMENDATIONS ADOPTED BY THE GENERAL COMMITTEE AT THE
BIRMINGHAM MEETING IN SEPTEMBER 1849.

Involving Application to Government.

That an application be made to Her Majesty's Government, to establish a Reflector not less than 3 feet in diameter, at the Observatory at the Cape of Good Hope, and to make such additions to the staff of the Observatory as may be necessary for its effectual working; and that the President be requested to communicate with Lord Rosse, Sir J. Herschel, the Astronomer Royal, Sir T. Brisbane, and Dr. Lloyd, on the subject, and to obtain the concurrence in the application, of the Royal and Astronomical Societies of London, the Royal Society of Edinburgh, and the Royal Irish Academy.

That an application be made to the Master-General of the Ordnance, to have the Levels of the Ordnance Survey of Ireland connected to the Mean Sea Level, as deduced by Mr. Airy (Astronomer Royal) from the Tide Observations round that Island; and that the President, Trustees and Officers of the British Association, and the President of the Royal and Geological Societies of London, and the Royal Irish Academy, be requested to make this application.

That application be made to the Master-General of the Ordnance, to have the British Arc of the Meridian published in its full extent, and that the President, Trustees and Officers of the British Association, the Royal Societies of London and Edinburgh, the Royal Irish Academy, and the Royal Astronomical Society, be requested to make such application.

That the Members of the British Association who are also Members of the Legislature, be requested to act as a permanent Committee, to watch over the interests of Science, and to inspect the various measures from time to time introduced into Parliament likely to affect such interests; and that the Mar-

quis of Northampton, Lord Rosse, Lord Wrottesley, Lord Adare, M.P., Sir Philip Egerton, M.P., and Sir C. Lemon, M.P., be requested to organize such Committee.

Involving Grants of Money.

Sir John F. W. Herschel having reported that the Meteorological Observations made at Kew are peculiarly valuable, and likely to produce the most important results, the Committee resolved that the sum of £250 be voted for the continuance of that establishment for the ensuing year; and that the sum be placed at the disposal of the Council, to whom the requisite arrangements are entrusted.

That three standard Barometers and other Meteorological Instruments be sent out to the British Consul-General at the Azore Islands, with the view of encouraging that gentleman (Mr. C. Hunt) to pursue his Meteorological Observations at the several Islands at which he has British Vice-Consuls; and that Colonel Reid, Colonel Sabine, Sir W. S. Harris, and Professor Phillips be a Committee for carrying out the above objects, with the sum of £25 at their disposal for the purpose.

Dr. Percy and Professor Miller.—To continue Researches on Crystalline Slags, with £10 at their disposal.

Dr. Schunck.—To continue Investigations on Colouring Matters, with £5 at his disposal.

Dr. Smith (Manchester).—To continue Investigations on the Air and Water of Towns, with £5 at his disposal.

R. Mallet, Esq., Rev. Dr. Robinson, Rev. Prof. Lloyd, and Prof. Oldham.—To determine by Instruments the Elements of the Transit of Natural and Artificial Earthquake Waves, with £50 at their disposal.

Dr. Lankester, Professor Owen, and Mr. R. Taylor.—On Periodical Phenomena of Animals and Vegetables, with £10 at their disposal.

Mr. Strickland, Dr. Daubeny, Professor Lindley, Professor Henslow.—On Vitality of Seeds, with £6 at their disposal.

Professor E. Forbes and a Committee.—To procure a Report on British Annelida, with £10 at their disposal.

Not involving Grants of Money, or Applications to Government.

That Professor Powell's Communication on Meteors be printed among the Reports, and be continued from time to time.

That a Committee be appointed for each Section, consisting of the President of the Section, with two other Members to be named by him (and the General and Assistant General Secretaries *ex officio*), for the purpose of revising the Recommendations which have from time to time been sanctioned by the Association, on subjects which are taken into consideration by the Section, respectively, and of reporting to the Council the steps which, in their opinion, should now be taken to give them the effect which Science requires.

That the Council be authorized to institute such steps as appear requisite to carry out this object.

That Meteorologists should be invited to communicate as they occur, to the Association, through the Assistant General Secretary, any Abnormal or other Meteorological Phenomena of interest observed by them.

That a Committee, consisting of Lord Adare, Dr. Robinson, Professor Forbes, Colonel Sabine, Colonel Reid, Professor Powell, Professor Challis, Sir J. Lubbock, Professor Chevalier, Mr. Birt, Mr. A. Smith, Mr. J. A. Brown, and Professor Phillips, with power to add to their number, be appointed to consider the best mode of promoting the observation of Luminous Meteors and Auroras; and that observers be requested to communicate with Professor Powell on Meteors, and with Professor Phillips on Auroras.

That a Committee, composed of Sir H. T. De la Beche, Sir W. Hooker, Dr. Daubeny, Mr. Henfrey, and Mr. Hunt, be requested to continue their investigations on the action of Carbonic Acid on the growth of ferns.

That Mr. R. Hunt be requested to furnish to the next Meeting a Report on the present state of our knowledge of the Chemical Action of the Solar Radiations.

That Mr. Mallet be requested to complete his Report on the Statical and Dynamical effects of Earthquakes.

That Professor E. Forbes, Dr. Playfair, and Dr. Carpenter, be requested to report on the Perforating Apparatus of Mollusca.

That the subject of Luminosity in Living Animals be recommended to the attention of Naturalists, with a view to determine the causes of such luminosity, the circumstances, the species of animals which possess it, and the state of knowledge on the subject; and that Mr. Darwin be requested to collect and receive observations on the subject.

That Mr. Henfrey be requested to report on the Hybridism of Plants.

That G. R. Porter, Esq., Colonel Sykes, Mr. Tooke, Professor Longfield, Mr. Lawson, and Professor Hancock, be requested to prepare a Report on the State and Progress of Statistics.

That the Communication of Lord Rosse, on Nebulæ, be printed entire among the Reports.

That Mr. Nasmyth be requested to prepare a Report on the Use and Relative value of the Hydrocarbons as a Lubricating Material; and that Dr. Playfair be requested to co-operate with him.

That it be recommended to the Council to consider of the propriety of reducing the number of copies to be printed of the next volume, and that the Council be authorized to arrange for the proper distribution of the unsold copies of previously published volumes.

Synopsis of Grants of Money appropriated to Scientific Objects by the General Committee at the Birmingham Meeting in September 1849, with the Name of the Member, who alone or as the First of a Committee, is entitled to draw for the Money.

	<i>Kew Observatory.</i>	£	s.	d.
At the disposal of the Council for defraying Expenses		250	0	0
<i>Mathematical and Physical Science.</i>				
REID, Colonel—Meteorological Observations at the Azore Islands		25	0	0
<i>Chemical Science.</i>				
PERCY, Dr.—Researches on Crystalline Slags		10	0	0
SCHUNCK, Dr.—Investigations on Colouring Matters		5	0	0
SMITH, Dr.—Investigations on the Air and Water of Towns		5	0	0
<i>Geology.</i>				
MALLET, R.—To determine by instruments the Elements of the Transit of Natural and Artificial Earthquake Waves		50	0	0
<i>Natural History.</i>				
STRICKLAND, H. E.—Vitality of Seeds		6	0	0
LANKESTER, Dr.—Periodical Phænomena of Animals and Vegetables		10	0	0
FORBES, Prof. E.—Report on British Annelida		10	0	0
Total of Grants		£371	0	0

General Statement of Sums which have been paid on Account of Grants for Scientific Purposes.

1834.							
	£	s.	d.		£	s.	d.
Tide Discussions	20	0	0	Brought forward	308	1	10
				Railway Constants	41	12	10
				Bristol Tides	50	0	0
1835.				Growth of Plants	75	0	0
Tide Discussions	62	0	0	Mud in Rivers	3	6	6
British Fossil Ichthyology	105	0	0	Education Committee ..	50	0	0
	£167	0	0	Heart Experiments....	5	3	0
				Land and Sea Level ..	267	8	7
1836.				Subterranean Tempera-			
Tide Discussions	163	0	0	ture	8	6	0
British Fossil Ichthyology	105	0	0	Steam-vessels	100	0	0
Thermometric Observa-				Meteorological Commit-			
tions, &c.	50	0	0	tee	31	9	5
Experiments on long-				Thermometers	16	4	0
continued Heat	17	1	0		£956	12	2
Rain Gauges	9	13	0				
Refraction Experiments	15	0	0	1839.			
Lunar Nutation	60	0	0	Fossil Ichthyology	110	0	0
Thermometers	15	6	0	Meteorological Observa-			
	£434	14	0	tions at Plymouth ..	63	10	0
				Mechanism of Waves ..	144	2	0
1837.				Bristol Tides	35	18	6
Tide Discussions	284	1	0	Meteorology and Subter-			
Chemical Constants ..	24	13	6	anean Temperature .	21	11	0
Lunar Nutation	70	0	0	Vitrification Experiments	9	4	7
Observations on Waves.	100	12	0	Cast Iron Experiments .	100	0	0
Tides at Bristol	150	0	0	Railway Constants	28	7	2
Meteorology and Subter-				Land and Sea Level ..	274	1	4
anean Temperature .	89	5	0	Steam-Vessels' Engines.	100	0	0
Vitrification Experiments	150	0	0	Stars in Histoire Céleste	331	18	6
Heart Experiments....	8	4	6	Stars in Lacaille	11	0	0
Barometric Observations	30	0	0	Stars in R.A.S. Catalogue	6	16	6
Barometers	11	18	6	Animal Secretions	10	10	0
	£918	14	6	Steam-engines in Corn-			
				wall	50	0	0
1838.				Atmospheric Air	16	1	0
Tide Discussions	29	0	0	Cast and Wrought Iron.	40	0	0
British Fossil Fishes ..	100	0	0	Heat on Organic Bodies	3	0	0
Meteorological Observa-				Gases on Solar Spec-			
tions and Anemometer				trum	22	0	0
(construction)	100	0	0	Hourly Meteorological			
Cast Iron (strength of) .	60	0	0	Observations, Inver-			
Animal and Vegetable				ness and Kingussie ..	49	7	8
Substances (preserva-				Fossil Reptiles	118	2	9
tion of)	19	1	10	Mining Statistics	50	0	0
Carried forward	£308	1	10		£1595	11	0

	£	s.	d.
1840.			
Bristol Tides	100	0	0
Subterranean Tempera- ture	13	13	6
Heart Experiments....	18	19	0
Lungs Experiments ..	8	13	0
Tide Discussions.....	50	0	0
Land and Sea Level ..	6	11	1
Stars (Histoire Céleste)	242	10	0
Stars (Lacaille)	4	15	0
Stars (Catalogue)	264	0	0
Atmospheric Air.....	15	15	0
Water on Iron.....	10	0	0
Heat on Organic Bodies	7	0	0
Meteorological Observa- tions	52	17	6
Foreign Scientific Me- moirs	112	1	6
Working Population ..	100	0	0
School Statistics	50	0	0
Forms of Vessels	184	7	0
Chemical and Electrical Phænomena.....	40	0	0
Meteorological Observa- tions at Plymouth ..	80	0	0
Magnetical Observations	185	13	9
	<u>£1546</u>	<u>16</u>	<u>4</u>

1841.			
Observations on Waves.	30	0	0
Meteorology and Subter- ranean Temperature .	8	8	0
Actinometers	10	0	0
Earthquake Shocks ..	17	7	0
Acrid Poisons	6	0	0
Veins and Absorbents..	3	0	0
Mud in Rivers.....	5	0	0
Marine Zoology	15	12	8
Skeleton Maps	20	0	0
Mountain Barometers..	6	18	6
Stars (Histoire Céleste).	185	0	0
Stars (Lacaille)	79	5	0
Stars (Nomenclature of)	17	19	6
Stars (Catalogue of) ..	40	0	0
Water on Iron.....	50	0	0
Meteorological Observa- tions at Inverness ..	20	0	0
Meteorological Observa- tions (reduction of) ..	25	0	0
Carried forward	£539	10	8

Brought forward	539	10	8
Fossil Reptiles	50	0	0
Foreign Memoirs	62	0	0
Railway Sections	38	1	6
Forms of Vessels	193	12	0
Meteorological Observa- tions at Plymouth ..	55	0	0
Magnetical Observations	61	18	8
Fishes of the Old Red Sandstone	100	0	0
Tides at Leith.....	50	0	0
Anemometer at Edin- burgh	69	1	10
Tabulating Observations	9	6	3
Races of Men.....	5	0	0
Radiate Animals.....	2	0	0
	<u>£1235</u>	<u>10</u>	<u>11</u>

1842.			
Dynamometric Instru- ments	113	11	2
Anoplura Britannicæ ..	52	12	0
Tides at Bristol	59	8	0
Gases on Light	30	14	7
Chronometers	26	17	6
Marine Zoology	1	5	0
British Fossil Mammalia	100	0	0
Statistics of Education..	20	0	0
Marine Steam-vessels' Engines	28	0	0
Stars (Histoire Céleste)	59	0	0
Stars (British Associa- tion Catalogue of) ..	110	0	0
Railway Sections.....	161	10	0
British Belemnites	50	0	0
Fossil Reptiles (publica- tion of Report)	210	0	0
Forms of Vessels.....	180	0	0
Galvanic Experiments on Rocks	5	8	6
Meteorological Experi- ments at Plymouth..	68	0	0
Constant Indicator and Dynamometric Instru- ments	90	0	0
Force of Wind.....	10	0	0
Light on Growth of Seeds	8	0	0
Vital Statistics	50	0	0
Vegetative Power of Seeds	8	1	11
Carried forward	£1442	8	8

	£	s.	d.
Brought forward	1442	8	8
Questions on Human Race	7	9	0
	<u>£1449</u>	<u>17</u>	<u>8</u>

1843.

Revision of the Nomenclature of Stars	2	0	0
Reduction of Stars, British Association Catalogue	25	0	0
Anomalous Tides, Frith of Forth	120	0	0
Hourly Meteorological Observations at Kingussie and Inverness	77	12	8
Meteorological Observations at Plymouth ..	55	0	0
Whewell's Meteorological Anemometer at Plymouth	10	0	0
Meteorological Observations, Osler's Anemometer at Plymouth ..	20	0	0
Reduction of Meteorological Observations ..	30	0	0
Meteorological Instruments and Gratuities	39	6	0
Construction of Anemometer at Inverness ..	56	12	2
Magnetic Co-operation	10	8	10
Meteorological Recorder for Kew Observatory	50	0	0
Action of Gases on Light Establishment at Kew Observatory, Wages, Repairs, Furniture and Sundries	133	4	7
Experiments by Captive Balloons	81	8	0
Oxidation of the Rails of Railways	20	0	0
Publication of Report on Fossil Reptiles	40	0	0
Coloured Drawings of Railway Sections....	147	18	3
Registration of Earthquake Shocks	30	0	0
Report on Zoological Nomenclature	10	0	0
Carried forward	<u>£977</u>	<u>6</u>	<u>7</u>

	£	s.	d.
Brought forward	977	6	7
Uncovering Lower Red Sandstone near Manchester.....	4	4	6
Vegetative Power of Seeds	5	3	8
Marine Testacea (Habits of)	10	0	0
Marine Zoology	10	0	0
Marine Zoology	2	14	11
Preparation of Report on British Fossil Mammalia	100	0	0
Physiological operations of Medicinal Agents	20	0	0
Vital Statistics.....	36	5	8
Additional Experiments on the Forms of Vessels	70	0	0
Additional Experiments on the Forms of Vessels	100	0	0
Reduction of Observations on the Forms of Vessels	100	0	0
Morin's Instrument and Constant Indicator ..	69	14	10
Experiments on the Strength of Materials	60	0	0
	<u>£1565</u>	<u>10</u>	<u>2</u>

1844.

Meteorological Observations at Kingussie and Inverness.....	12	0	0
Completing Observations at Plymouth.....	35	0	0
Magnetic and Meteorological Co-operation..	25	8	4
Publication of the British Association Catalogue of Stars	35	0	0
Observations on Tides on the East Coast of Scotland	100	0	0
Revision of the Nomenclature of Stars.. 1842	2	9	6
Maintaining the Establishment in Kew Observatory.....	117	17	3
Instruments for Kew Observatory	56	7	3
Carried forward	<u>£384</u>	<u>2</u>	<u>4</u>

	£	s.	d.
Brought forward	384	2	4
Influence of light on Plants	10	0	0
Subterraneous Tempera- ture in Ireland.	5	0	0
Coloured Drawings of Railway Sections....	15	17	6
Investigation of Fossil Fishes of the Lower Tertiary Strata	100	0	0
Registering the Shocks of Earthquakes, 1842	23	11	10
Researches into the Structure of Fossil Shells	20	0	0
Radiata and Mollusca of the Ægean and Red Seas.....1842	100	0	0
Geographical distribu- tions of Marine Zo- ology	0	10	0
Marine Zoology of De- von and Cornwall ..	10	0	0
Marine Zoology of Corfu	10	0	0
Experiments on the Vi- tality of Seeds.....	9	0	3
Experiments on the Vi- tality of Seeds..1842	8	7	3
Researches on Exotic Anoplura.....	15	0	0
Experiments on the Strength of Materials	100	0	0
Completing Experiments on the Forms of Ships	100	0	0
Inquiries into Asphyxia	10	0	0
Investigations on the in- ternal Constitution of Metals.....	50	0	0
Constant Indicator and Morin's Instrument, 1842	10	3	6
	<u>£981</u>	<u>12</u>	<u>8</u>

1845.

Publication of the British Association Catalogue of Stars	351	14	6
Meteorological Observa- tions at Inverness ..	30	18	11
Magnetic and Meteor- ological Co-operation	16	16	8
Carried forward	<u>£399</u>	<u>10</u>	<u>1</u>

	£	s.	d.
Brought forward	399	10	1
Meteorological Instru- ments at Edinburgh	18	11	9
Reduction of Anemome- trical Observations at Plymouth.....	25	0	0
Electrical Experiments at Kew Observatory	43	17	8
Maintaining the Estab- lishment in Kew Ob- servatory	149	15	0
For Kreil's Barometro- graph	25	0	0
Gases from Iron Fur- naces	50	0	0
Experiments on the Ac- tinograph.....	15	0	0
Microscopic Structure of Shells	20	0	0
Exotic Anoplura..1843	10	0	0
Vitality of Seeds..1843	2	0	7
Vitality of Seeds..1844	7	0	0
Marine Zoology of Corn- wall	10	0	0
Physiological Action of Medicines	20	0	0
Statistics of Sickness and Mortality in York ..	20	0	0
Registration of Earth- quake Shocks ..1843	15	14	8
	<u>£831</u>	<u>9</u>	<u>9</u>

1846.

British Association Ca- talogue of Stars, 1844	211	15	0
Fossil Fishes of the Lon- don Clay	100	0	0
Computation of the Gaus- sian Constants for 1839	50	0	0
Maintaining the Estab- lishment at Kew Ob- servatory	146	16	7
Experiments on the Strength of Materials	60	0	0
Researches in Asphyxia	6	16	2
Examination of Fossil Shells	10	0	0
Vitality of Seeds..1844	2	15	10
Vitality of Seeds..1845	7	12	3
Marine Zoology of Corn- wall	10	0	0
Carried forward	<u>£605</u>	<u>15</u>	<u>10</u>

	£	s.	d.		£	s.	d.
Brought forward	605	15	10	1848.			
Marine Zoology of Britain	10	0	0	Maintaining the Establishment at Kew Observatory	171	15	11
Exotic Anoplura.. 1844	25	0	0	Researches on Atmospheric Waves	3	10	9
Expenses attending Anemometers	11	7	6	Vitality of Seeds	9	15	0
Anemometers' Repairs .	2	3	6	Completion of Catalogues of Stars	70	0	0
Researches on Atmospheric Waves	3	3	3	On Colouring Matters ..	5	0	0
Captive Balloons.. 1844	8	19	8	On Growth of Plants ..	15	0	0
Varieties of the Human Race	7	6	3		<u>£275</u>	<u>1</u>	<u>8</u>
Statistics of Sickness and Mortality at York ..	12	0	0	1849.			
	<u>£685</u>	<u>16</u>	<u>0</u>	Electrical Observations at Kew Observatory	50	0	0
1847.				Maintaining Establishment at ditto	76	2	5
Computation of the Gaussian Constants for 1839	50	0	0	Vitality of Seeds.....	5	8	1
Habits of Marine Animals	10	0	0	On Growth of Plants..	5	0	0
Physiological Action of Medicines	20	0	0	Registration of Periodical Phænomena	10	0	0
Marine Zoology of Cornwall.....	10	0	0	Bill on account of Anemometrical Observations	13	9	0
Researches on Atmospheric Waves.....	6	9	3		<u>£159</u>	<u>19</u>	<u>6</u>
Vitality of Seeds.....	4	7	7				
Maintaining the Establishment at Kew Observatory.....	107	8	6				
	<u>£208</u>	<u>5</u>	<u>4</u>				

Extracts from Resolutions of the General Committee.

Committees and individuals, to whom grants of money for scientific purposes have been entrusted, are required to present to each following meeting of the Association a Report of the progress which has been made; with a statement of the sums which have been expended, and the balance which remains disposable on each grant.

Grants of pecuniary aid for scientific purposes from the funds of the Association expire at the ensuing meeting, unless it shall appear by a Report that the Recommendations have been acted on, or a continuation of them be ordered by the General Committee.

In each Committee, the Member first named is the person entitled to call on the Treasurer, John Taylor, Esq., 6 Queen Street Place, Upper Thames Street, London, for such portion of the sum granted as may from time to time be required.

In grants of money to Committees, the Association does not contemplate the payment of personal expenses to the Members.

In all cases where additional grants of money are made for the continuation of Researches at the cost of the Association, the sum named shall be deemed to include, as a part of the amount, the specified balance which may remain unpaid on the former grant for the same object.

General Meetings (in the Town Hall).

On Wednesday, September 12th, at 8 P.M., the late President, The Marquis of Northampton, V.P.R.S., resigned his Office to the Rev. T. R. Robinson, D.D., M.R.I.A., who took the Chair at the General Meeting, and delivered an Address, for which see p. xxix.

On Thursday, September 13th, the power of Mr. Gassiot's Battery in exciting Light and Heat was exhibited, and Dr. Faraday added some explanation of the Phænomena.

On Monday, September 17th, the Rev. Prof. Willis, M.A., F.R.S., gave a Discourse and exhibited Working Models, to illustrate the result of some recent Experiments on the Transit of different Weights with varying Velocities on Railways.

On Wednesday, September 19th, the concluding General Meeting of the Association was held, when the Proceedings of the General Committee, and the grants of money for scientific purposes were explained to the Members.

The Meeting was then adjourned to Edinburgh in August 1850*.

* The Meeting is appointed to take place on Wednesday, 31st July.

A D D R E S S

BY

THE REV. THOMAS ROMNEY ROBINSON, D.D.,

M.R.I.A., F.R.A.S.

GENTLEMEN,—If I thought only of myself, the embarrassment which in taking the place to which you have called me I feel, would be much increased by the way in which my predecessor has spoken of me. Hitherto it has been filled by men illustrious in the senate or the field, heads of the societies which are the centres of our scientific life, and lodestars of the great institutions which have been through many ages the guides of our nation in the progress of intellectual cultivation. Against such men, if I weigh myself, I know how much I shall be found wanting. But I trust I may be permitted to regard myself as the type of a humbler but not useless class, for whom this Association was especially devised, and whom it enables to add their tribute to swell the general store. For it is not like the forbidden ground of romance, a region where heroes only can tread; it is not a mere instrument for collecting into a focus the light of the suns of the intellectual sky. It is rather like those machines which unite the power of many; singly weak, but achieving by the union works which would overtask the strength of the mightiest individual. In one thing only I will venture to take to myself as not unmerited, the praise of Lord Northampton. In zeal for the welfare of this Association, in intense interest for the accomplishment of its objects I yield to none; and if these may suffice, I hope I shall not be found unworthy of the trust you repose in me.

Yet, it is no common responsibility with which you have charged me; for this Association is one of the great powers which the altering phases of the world have called into action. But a few years since it could not have existed; and even now some persons are found unable to appreciate its

results or understand its purpose. In fact, as the invention of a new machine or process of manufacture is evidence that the old is becoming inadequate to meet the demand which it formerly satisfied; so the feelings which have so successfully called into being our Association here, and similar institutions abroad, may be regarded as a proof that the existing agencies for the development of scientific knowledge were becoming unequal to their work, and that some higher power must be sought, of energy commensurate to the increasing pressure. Such a power, I think, it is now certain that we afford. It is possible that the form of this great experiment may receive some modification; for example, that it may involve a yet wider application of the mighty principle on which it is based, and become a union not only of persons but of institutions. But we have established beyond doubt that it is a trial in the right direction,—that its principle is the true one, the principle of Association. It may perhaps seem trivial to attach importance to such an assertion; in commercial enterprise, in manufactures, in politics its truth is universally confessed; what then is there new in applying it to science? Nothing, assuredly: in fact science, at least physical science, owes to it almost its very existence, and certainly its progress; and the wonder is that none seem to have fully comprehended this before the founders of the British Association. Observe, that though physical science is of recent birth, physical knowledge has been an object of desire from the very origin of our race. Some have followed it for the sake of the powers which it conferred; and some from the high instinct which reveals to a noble mind the beauty and majesty of such pursuits. In the first glimmer of history, the astronomy of the Assyrian Magi looms through the darkness; the geometry which might have been its champion and guide appears in no feeble development even in the fabulous antiquity of India. The sepulchres of Etruria and Egypt, the palaces of Nineveh, are giving up to us relics of art that imply in high perfection the existence of that practical chemistry which was transmitted to us through their Arabian successors. When we look at the marvellous architecture of the middle ages, we find a mastery of the principles of equilibrium and pressure, that fills the mind capable of appreciating it with delight beyond even what its surpassing beauty inspires; and we know from the writings of Roger Bacon and Kircher that many facts of experimental physics were current in the cloister. The elements were in existence, but some power was wanting which could combine them into a body and give it life. That power was free, open, honest association. Not intellectual energy or acuteness: the Greeks possessed *that* to an extent never perhaps equalled by any other people; but they were made incapable of steady union for any purpose, by the strange elements of repulsion which seemed inherent in their nature and

split their philosophers into sects, their nation into fragments. Elsewhere the separation was still wider; the priestly casts of old, the conventional clergy and masonic societies of more recent times, could not coalesce with the rest of the world in the union which I hold to be essential to the growth of science. Therefore, however extensive their knowledge (and they knew more than is generally supposed) it never ripened into general principles; it even became corrupt in the dull stagnation of the mystery in which it was buried,—an instrument of superstition or imposture, a delusion to its possessors themselves. Astronomy became astrology,—chemistry, alchemy—natural philosophy, magic. Brewster has shown how the concave mirror brought up an apparition when it was needed, and Boutigny has revealed how the repulsive energies of heat ministered to the iniquity of the ordeal. But this period of isolated labour, under which the intellectual domain of our race lay so long fallow, closed at last; and the principle of association revealed itself, at one of the epochs of that movement which from time to time stirs up the region of mind, as those of geology do the earth at the commencement of some great formation. To borrow from that science an illustration,—the reign of reptiles and monsters gave way to higher beings that soared in the sky; the dominion of Aristotle and the schoolmen disappeared before the age of Copernicus, Kepler, Galileo, and Bacon. From the fifteenth century downwards we find the philosophers of Europe beginning to be worthy of that name, lovers of knowledge. Instead of wrapping up their discoveries in secrecy, using them as a means of influence over the ignorant, or brooding on them as food for haughty self-love; we find them forming a brotherhood of knowledge,—eager to communicate their inventions, applying to each other for instruction, and even disputing among themselves the honour of priority in successful research. If the Florentine astronomer still envelopes in cipher his observations of Venus and Saturn, it is lest a rival should anticipate what was necessary to perfect his discovery:—while the Monk of Oxford hides in a similar veil his knowledge of gunpowder to exalt himself in the opinion of the world, yet keep his secret. The step in advance was wide, and the onward progress was rapid. It is not merely that each discovery, which is thus freely communicated, becomes an imperishable addition to the treasury of human knowledge, but it is also a source of others, more numerous as it is more widely diffused,—like a syngenesious flower, whose winged seeds would produce little if confined to the neighbourhood of their parent, but bear a thousand-fold when scattered over the land. He who first finds a physical fact or principle often fails to trace it to its full extent: pre-occupied by some particular object of research, led by special views, he looks at it with reference to them alone,—and were he

sole labourer in the mine, much of its wealth would be lost : it may be too vast to be explored by the power of one mind, or within the limits of one life ; or it may require aids and appliances which solitary individuals do not possess : to say nothing of what is still more important,—the increase of energy which flows from the sympathy and admiration of a multitude. It is not too much to say, that the progress of mankind in science during the two centuries to which I refer, far exceeded what had been made during the fifty-six that preceded them : yet the force which impelled it was only partially and imperfectly exerted, and it was soon felt to be capable of far wider application. In this stage of its action the principle of association had operated on only a few mighty spirits whom the sense of kindred pursuits and powers linked together ; but from whom their very transcendence kept their humbler fellows at a reverential distance. It was necessary that these also should be included in its bond :—and the age of Societies began. By condensing into a multitude of local centres the activity which was weakened in its diffusion, that privilege of labouring to extend the boundaries of knowledge, which had been the glory of a chosen few, was extended to a multitude ; societies devoted to this object arose in different countries, varying in constitution and form, but all emanating from the same necessity of bringing united exertion to bear on what every passing year showed to be among the noblest objects of human existence. And in this they were eminently successful :—strong in numbers, they were stronger in local concentration ; their definite and permanent organization was a source of life and power ; and the visible results of their activity were manifest to the world. In many instances they acquired a legal and corporate existence, which gave them a hold on general opinion and even on governments ; their pecuniary resources and moral weight afforded them the means of researches beyond the reach of ordinary inquirers ; and their exclusive character, whether limited by election or by appointment, by making it an object of ambition to belong to them, gained for their pursuits a popularity which their intrinsic worth might not so soon have won. A still more—perhaps their most—important feature is the principle of systematic publication, the value of which has gone on increasing to the present hour, and cannot be overrated. Their Transactions gave to the world not merely casual observations, which might otherwise have perished, but elaborate investigations, which probably would never have found a publisher in the ordinary course of trade,—perhaps never have been undertaken had not this channel been open to their authors. It would be foreign to my purpose, even were it possible, to give you an account of the philosophical societies which have flourished, not merely in Europe, but in some of the most distant regions which her sons have reached as colonists

or conquerors. A description of them would fill volumes,—a record of their proceedings would be the history of scientific progress for the last two centuries:—I might say of science itself, for, in fact, they began with Newton, and he stands like the sun in Heaven; all is luminous after he has risen, all before darkness or twilight. Yet, while rendering to them the praise which their services have so well deserved, we must not forget that as they were called into existence to meet a state of things which has passed away, so the altered condition of the human mind requires from them now a very different class of function from those which they discharged at first; and that circumstances may occur in which they may retard instead of advancing the progress of knowledge. That which I referred to as an original element of their power is of this number,—their restricted and local character: their very nature requires that they be placed in large towns or cities, and they cannot multiply their members beyond narrow limits. This was not injurious as long as a single room in a tavern was sufficient to hold all the philosophers of the metropolis, or the means of experiment and instruction were scarcely accessible out of its precincts. It is far otherwise now,—when we count more thousands, and those, too, of higher standard in the ranks of science, than units could be reckoned at the beginning of last century, and when every day adds to their number. No possible extension of the great societies can meet this, even were they disposed to make it,—which I believe they are not. On the contrary, there is among them a tendency to limit their admissions to men of high fame and proved attainments, and thus, in some degree, form an Aristocracy of Science. What, then, is to become of the rest?—are they to form provincial societies similarly organized? This, it seems to me, is but a retrograde step; a violation of the great principle to which we owe our advance,—a breaking up into fragments of the energy which it should be the aim of all our efforts to associate into one mighty unity; and however valuable such societies are as auxiliaries, unless it be found possible to link them, by some principle of federation, unto our great body, without interfering with their self-government and independence, I feel that much of the good which they are capable of effecting must be lost. Secondly, the increasing vastness of the field which we have to cultivate surpasses the powers of any single body of labourers. Look, for instance, at the most illustrious of all, the Royal Society. At first, as we see from its *Transactions*, it was a mere collector of detached facts and observations, and for them took even a wider range than is attempted by all our Sections; it collected too, with but little discrimination:—in that dawning of information it was not always possible to distinguish a pebble from a pearl. It soon, however, became fastidious; for it reached the point when it became more im-

portant to class and interpret than to collect; and the latter part of its office became subordinate to the other. By degrees, as its accumulating duties began to surpass its powers, we find dissatisfaction appearing, and complaints that particular branches of science are neglected to favour others not so important. At last, the necessity of a division of work becomes apparent; a society splits off to devote itself to geology,—another to astronomy,—others to various branches of Natural History,—while the parent, like Trembley's hydra, is more active and powerful than before this division. That this process has increased our knowledge a hundred-fold, will not be disputed by any who have watched its progress during the last thirty years; and yet it can scarcely be denied that, besides the chance of exciting hostile feelings between rival societies, it is open to another objection. The different branches of science cannot well be isolated; each depends on many others. Geology presses into its service not merely its special subject, but also the Geometry of Hopkins, the Botany of Lindley, and the Zoography of Owen and Agassiz. Astronomy must not only track the unseen with Adams and Le Verrier, or fathom the abysses of the sky with Herschel and Rosse,—it must also visit the workshops of the machinist with Airy and Struve. And so of the rest; they cannot be disunited: and therefore it is evident that some system must be found, which, while it leaves unfettered the whole special organization of each Society, shall yet combine their exertions, not merely with each other, but also with the great and ever-increasing multitude of fellow-labourers beyond their precincts. Therefore it was not merely a happy thought of the good and wise men who were the founders of the British Association which led to its existence; this, or something equivalent, was a necessary result of the expansion of that principle whose course I have been tracing, and which must, ere long, have found some other development had they not turned it in this direction. It leaves untouched all that was previously available, and merely adds what experience had shown to be deficient. Thus we do not interfere in any way with any Society; on the contrary, we identify ourselves with them as far as possible. We admit, *as of right*, the members of all chartered Societies that publish Transactions throughout our empire; the officers and councils of philosophical institutions, and all their members who are recommended by those councils; and our governing power, or General Committee, is almost entirely derived from the same source,—it is chiefly composed of “members who have printed papers in the Transactions of any philosophical society, or of delegates from such societies or philosophical institutions.” We withdraw nothing from their Transactions; our reports are of a totally different character; on the contrary, we assist them; for many of the most valuable communications, which

those publications contain in latter years, have originated in the proceedings of our Sections. Yet, though we have so much in common with them, it would be a gross error to confound us with them, or to imagine that any increase of their activity or any change in their management could supersede Our Office. Not the least important part of it refers to persons entirely unconnected with them, persons who have struggled after knowledge in difficulty and obscurity, whose diffidence would shrink from the distinction belonging to such connection, or even who, without any scientific acquirements, have yet a reverence for them, a perception of their worth. Such we can count by thousands; and every one of them, I am confident, has been profited by the influence which we have exerted on his mind. We have gone still further, and admitted ladies as Associates; exciting the surprise and perhaps scorn of those who think women fit only for household cares or showy accomplishments: and we have done well; for without referring to Mrs. Somerville, Mrs. Marcet, or others whom I would name were they not present, I have myself known some whose proficiency in several of our departments might have put many an F.R.S. to shame, who were not to be surpassed in all the graces of their sex, and were perfect in all the relations of domestic life. Man cannot ascend in the scale of intellectual power unless woman rises with him. Another advantage which we possess above stationary societies is, our mobility; we can pursue our labours wherever much is to be learnt or many are to be taught. From the Universities, the seats of abstract science, we have ranged to the mighty emporia of Great Britain, to the treasure-houses of its mineral and metallurgic wealth, to the marvellous palaces of its industrial art; and at every step of our progress, even the most highly gifted and richly stored among us have learned new facts, seen opening before them new lines of thought, and met new men. It is a glorious discipline, the very one which Homer attributes to the wisest of his heroes: *πολλῶν ἀνθρώπων ἶδεν ἄστεα καὶ νόον ἔγνω*. And let us hope, that, in the expressive imagery of the New Atlantis, we also may be “dowry men” and “merchants of light;” that they whose seats become the marts of our intellectual commerce may receive in it their share of the illumination which we seek; and that by imparting to them new ideas, by correcting error, by opening to them more fully the laws which rule those elemental powers that serve them in works of microscopic beauty or giant might—we may endow them with gifts which shall both increase the reward of their own industry and enterprise, and augment the prosperity and glory of our country.

Our Association has been tried during eighteen years, and with a success which has exceeded by far what its most ardent friends had ventured to

anticipate. It would of course be unreasonable to expect that its career should be at all times equally brilliant, or that an institution, whose roots spread into every part of the realm, and every order of its people, can be free from the fluctuations by which their prosperity is ruffled. It must also be borne in mind, that if we adhere, as I trust we ever shall do, to our rule of assembling wherever we are called by the interests of science, we must occasionally visit remote and unattractive localities, where the difficulties of access, and the want of accommodation will discourage many from attending. But yet we can truly assert, that in *each* of these eighteen years—and assuredly this nineteenth will be no exception—it has added largely to our knowledge, and in no respect fallen short of the objects contemplated by its founders. These were, as stated to the original meeting at York, “to promote the intercourse of the cultivators of science with each other, and with foreign philosophers;” “to give a stronger impulse, and more systematic direction to scientific inquiry;” “to obtain a greater degree of national attention to the objects of science, and a removal of the disadvantages which impede its progress.”

Let me request your attention for a few moments respecting each of these. The first of them may perhaps be undervalued by some, or rated merely as an intellectual luxury. Even at that low estimate, it is above price; but it is of far higher import. If to visit the field of some high deed—to stand before the sepulchre of the illustrious dead—can fill the mind with lofty aspirations, and lift it to the noblest emprise, how much more shall it kindle in the actual presence of one of those great beings who are raised up by our Heavenly Father to be the lights of our race! Who could stand before Bessel without feeling how genius is exalted by industry! What a lesson of truth and decision was written on the brow of Dalton! But our close intercourse with each other is still more precious, from its tendency to check some evil elements of our nature. For instance, the bitter disputations and petty hostility, which have too often disgraced the records of science, and made its followers contemptible. The most irritable man must feel less disposed to apply violent language, or attribute unworthy motives to one whom he has met in kindly intercourse, and whose character he has appreciated, than when he encounters a perfect stranger in the arena of the press; or if he have offended, how many opportunities of atonement and reconciliation are offered by a reunion like this! Accordingly, this fault has nearly disappeared; and when traces of it occur, it is only in persons who have not fully entered into the spirit of our Association. Nor is it less powerful to avert a still greater danger—the greatest, in fact, which besets our pursuits—that of self-esteem. The true philosopher does not incur it: he

knows too well the proportion between his ignorance and his knowledge ; but if there be any who, from being the wonder of a limited circle, or from exaggerating the difficulty of his own attainments, is disposed to exalt himself above his peers, let him visit us, and I will answer for his cure. There is not a man on earth who could try the experiment without finding superiors in some of our departments, and scarcely any who would not find an equal in that of which he is vain.

As to our foreign visitors, I need not take the trouble of proving what you all feel : the attracting them to our shores—the having the opportunity of knowing such men as Arago and Cæsted, Ritter, Encke or Struve, Bache or Henry—of strengthening by the ties of friendship that brotherhood of science, to which I have already referred as of such importance—that alone would be worth an Association to obtain it. Even on this, the first night of our meeting, we are honoured by several distinguished guests. On another occasion I shall express to them our acknowledgement of the honour with which their presence graces us ; but now shall refer only to one—the Chevalier Bunsen—in answer to any who may suppose that an attachment to any of the various branches of science, in which he is so highly gifted, unfits a man for the most energetic discharge of the active duties of public life.

In the second object—"to give a stronger impulse and more systematic direction to scientific inquiry"—we have not been less successful. The very excitement connected with our meetings, is itself such an impulse, and a most powerful one. Those of our members who have long been known as the chief ornaments of our great philosophical societies—devoted to science, and rich in its triumphs—feel it as fully now as when first they joined us. At each new occurrence they seem to find a renovation of enthusiasm—a flow of hope, an increase of resolution among us—which send them with fresh strength to resume their labours ; and will be present to them in the hours of despondency and gloom, which at times cloud even the firmest spirits, like a beam of light. Nor is our spell less potent on those yet untried in the race, who come forward to communicate the first fruits of their research—the truth which has rewarded their solitary toil. To such, the approbation, the kind advice, the affectionate warning of their more renowned companions, is like a horoscope that stamps the future course of life ; more powerful even than the applause of the multitude, who rejoice at the success of one unknown, and are encouraged by it to similar exertion. But still more precious is the excitement of plunging into this mighty flow of intellect, to one whose lot is like mine, cast remote from the resorts of science—with few or none near him to understand or value his pursuits ; nothing but his own fixity of resolve to disperse the listlessness which thus gathers on the mind and clogs its wing.

To him you are as an oasis to the travellers in the Desert, whose palms and fountains make him forget the waste which he has left, and store him for another journey with the means of life. But we not only give this impulse, we also guide it; and by guiding it, sustain and increase its strength, as well as by removing the difficulties which resist it. A small part of what we have thus accomplished you find in the volumes which we have published; the most important, as I already stated, is to be found in the Transactions of various Societies or in separate works. Let me select a few instances, for rapid notice, as time will fail for more. To begin with the science to which I myself am specially devoted—Astronomy: it has been above all others patronized by nations and individuals; in our own country a Society of high fame and influence has been established for its advancement; and yet it has remained for us to render it services of no common order, which I may be permitted to explain in some detail. In it, as in many other physical sciences, the observation of facts is merely the crude ore, which must be sorted, and sifted and passed through the furnace to make it yield the metal which we seek. The mere task of making the observations is generally a pleasure; but it is far otherwise with the subsequent process. The arithmetical operations which it requires, demand much more time and involve much more labour; that, too, rather intellectual, and involving at every step liabilities to error. Take a simple instance: you have determined with minute precision the apparent place of a star in the sky—if you stop then, you have done nothing. The place you have obtained is not the true one: the atmosphere has bent the line of sight; while the light travels down your telescope you and it have been moving; and the sky-marks by which you map the star are themselves disturbed by various and complicated motions. For all these you must allow; but to do so requires, on an average, even in the most improved method of modern times, the writing of 400 figures and the performance of 50 arithmetical operations. But the numbers themselves employed are the result of other complicated operations; nearly half are constant for the same star, but an equal number have relation to the sun and moon, and therefore vary from day to day. Were these also to be calculated, it would add an equal amount of work. But even this is insufficient, for we must compare what we thus obtain with the results of former astronomers; and this also cannot be done without bringing them together by the same arithmetic talisman; so that were the whole to be performed by the one calculator, I have found that, however expert he may be, he must expend an hour at least in obtaining each result. Now, from most of this drudgery in the case of more than 8000 stars, he is relieved by the Catalogue which the Association has given to the world. It contains for each the constants already

noticed ; and gives the prompt and easy means of making the comparison ; so easy, that probably before its epoch, 1850, is past, every one of those places will have been verified in the sky. Such an undertaking could have been effected only by such a power as ours, which could at once engage the services of such men as Baily, Herschel, Stratford and their fellow-labourers, and devote to the inferior part of the work an expenditure exceeding 2000*l*. In fact, had we done nothing else, I say fearlessly that this work alone would have secured us an enduring claim on the gratitude of science. Let me here remark, that there are many other departments in which we could render most important service by the mere collection of the Constants that belong to them ; as we have done in this case and in that of terrestrial magnetism. Constants are the framework of knowledge, the concentration of power ; they belong peculiarly to our domain, and were marked out as such long since ; but though unfortunately this work was not executed by that powerful mind to whom we entrusted it, I hope the subject will not be forgotten. I might tell you of the theory of the tides, which Laplace might well style “the most thorny of problems,” but of the greatest interest to a nation

“ Whose march is o’er the mountain wave,
Whose home is on the deep.”

I might tell you of light thrown on it by observations obtained by our influence, reduced at our expense, and unravelled by one worthy of going beyond the steps of Newton and Bernouilli. To the same philosopher we owe the execution of another important task,—the determination of the plane which marks the level of the sea unvarying with the changes of the tide ; a precious gift, as but for it in a few years the absolute levels of our great national surveys would have become a delusion. In Ireland, for example, they referred to the low-water of spring-tides ; a mark which could not be recovered, as it varies both with time and place. I know not whether this has been yet corrected, but I trust it soon will, as Airy’s observations afford the data. It would be tedious to tell you all of this kind that we have effected ; and I leave the subject, with a reference to one more example,—the investigation of the motion and nature of waves which we owe to Mr. Scott Russell. These lead by an unexpected line to one far more interesting in a practical view, the resistance and the form of ships. On this subject it appears that valuable information has been collected for us ; and it cannot but be matter of regret that materials obtained at so great an expenditure of money (more than 1000*l*.), of labour and thought should remain unavailable, especially considering the present imperfect condition of naval architecture in reference to science. In many instances we have aided inquiries of inestimable value,

though we did not originate them :—as the Fossil Ichthyology of Agassiz, and those of Owen on Fossil Reptiles and Mammalia, which perhaps but for us would never have been completed ; and in fine I may mention as an approximative measure of the impulse which we have given to science, that we have expended in this way 15,000*l*. Observe, too, that to this must be added whatever is the pecuniary value of the labours of those members of the Association who have given us their services. *That* all is gratuitous ; and if you consider who many of them are, you will find it not easy to assign its price. But I regard as even more conducive to the advancement of science, another part of our labours, peculiarly our own,—I mean the reports which place before us the actual boundaries of our knowledge. Much intellectual energy is wasted in inventing what is already known ; much spent on objects comparatively unimportant for want of a due estimate of their worth, many walks untrodden because it is supposed they have been sufficiently explored. For all this a remedy is found in those admirable surveys, so many of which are found in our volumes ; they are as it were a “taking stock” of our intellectual wealth, and tell us how much of it is real, how much doubtful, how much wanting. Whether we consider those which embrace a whole science, as those of Airy on astronomy or Forbes on meteorology,—or those which include some one of its divisions, as those of Sabine on terrestrial magnetism, Lloyd on physical optics, Rennie on hydraulics, those by the Dean of Ely and his compeers on parts of mathematical analysis, or those of Owen and his fellow-labourers in natural history, with a multitude of others,—it is scarcely possible to over-estimate their worth. You find there condensed into a few pages the essence of many volumes ; the chaos of clashing statements and conflicting opinions reduced to harmony and order ; truth winnowed from error, facts from conjecture. They place within the reach of the most secluded student, a treasure of certain information which it would be hard for him to obtain, even had he access to the libraries and institutions of the metropolis ; and even to the mind that is best stored they save time,—and time is power. Such reports we shall I trust continue to receive in increasing numbers ; and as long as we do, we prosper, for they are the surest index, though not the most showy, of our usefulness.

I have left myself but little space to consider how far we have fulfilled the third of our objects—“to obtain a greater degree of national attention to the objects of science.” Most assuredly it was needful ; for nowhere in the civilized world is less honour paid by a nation to science, though nowhere is national prosperity more connected with its progress, nowhere are heavier penalties paid for its neglect. I do not now refer to the remarkable fact that in Britain alone, men whose scientific fame fills all Europe were seldom thought

worthy of any honorary distinction by their government. As it relates to themselves, this is of no importance; but it is of deep concern to the honour of this country. The true votary of science loves it for itself: in its possession he has a higher honour, a nobler decoration than man can give. *He* does not require to be bribed to follow it by titles or ribbons,—the baits for meaner spirits, the lure to lower achievements. But he knows that though *he* despises such gauds, those who bestow them hold them precious; and they serve him as a scale, by which he finds that great men once placed a Herschel or a Brewster nearly on a level with a third-rate soldier, or the annual magistrate of some town that might be honoured with a royal visit. Nor do I refer to the miserable œconomy which permitted such men as Ivory and Dalton (to speak only of the dead) to waste, in the drudgery of earning a precarious subsistence, the years, the powers, the hopes which could have borne light into the remotest and darkest recesses of the realms of inquiry; though it does contrast painfully with the munificent provision which republican France, and despotic Russia, heap on such men when they can find them. Both these spring from the same root,—the gross ignorance in this department of intellect, which up to the beginning of this Association, and long afterwards, prevailed in the land. The industrial classes of our countrymen were wont to rely in their pursuits on the unenlightened dexterity and empirical success which resulted from experience, and to scoff at the idea of learning anything useful from a mere theorist; those, whom wealth and independence permitted to choose, seldom sought employment or pleasure in this unfashionable region, their education, though the best then current, having given them very little cognizance of what it might contain. And to ascend still higher, even to the executive and legislative bodies, they “cared still less for science;” the tension of political life engrossed all their faculties: they disliked philosophers as meddlers, or despised them as dreamers. The head of a great military department once said that he *hated* scientific officers! Any one of his engineers might have told him that more money had been wasted, and lives lost in that department, from sheer ignorance of science, than any one could think of without shame and sorrow. The question which I know to have been asked by another in “high places,” though milder in expression, was not less scornful—“Of what use is science?” He who asked it ought to have known better. Whatever tends to raise man above low and sensual pursuits—whatever to lead him from the partial and present to the general and the future—whatever to exalt in his mind the dominion of order and the supremacy of truth,—that must be useful to the individual, useful to the nation. Even had he been incapable of rising above the gross measure of pecuniary value, he ought to have been able to give a mighty answer to his own

inquiry. There is not a single element of our commercial prosperity in which the vivifying power of science might not be felt, in which the loss arising from want of that certainty of action which mere unenlightened practice can never attain, does not reach an amount which, if stated in figures, would astound the most thoughtless. For instance, the causes which in our great cities hasten the death and debase and embitter the life of so many, have at last been forced by chemists and physiologists on the notice of the public. Look at Dr. Smith's report on the air and water of towns, in this volume; and when we think that the victims of the deadly influences which are there revealed are chiefly found among the people whose industry is the foundation of our greatness,—that every year cut off from the life of each of these is so much subtracted from national wealth,—even were all moral sense or religious feeling dead in us, we must confess that the knowledge which is capable of averting them "is of use." And the ships that bear the treasures produced by this industry through the world are lost to a fearful amount—nearly *three* daily. What are they worth—ship, cargo, men? and most of them perish from want of nautical science or from unscientific construction. How many men have been ruined by searching for minerals, when the merest smattering of geology would have dispelled their delusion! On the other hand, the agricultural produce of our islands might be doubled by a more perfect application of the principles of botany and chemistry. The manufacture of iron has been augmented sixfold by the use of the puddling furnace and the hot-blast—both gifts of theory. How gigantic a result is this, without reference to the increase in the thousand arts of which this immense supply of that most precious of metals is the exponent! The splendid machinery in which we excel the world owes its present perfection to mechanics who are conspicuous in *our* Sections, to impulses given by philosophers like Willis or Babbage. Nay, the steam-engine itself, your immortal townsman's great conquest,—that earthly fate to which now seems to be committed the weaving of the world's destiny,—that itself was a pure induction of science:—and beyond *that* I need not go. But we live in better times; for no statesman now would be so imprudent as to ask such a question, even were there any so unfortunate as to think it, which I trust there are not. And this change we, the British Association, have in no small degree helped to produce. We have carried far and wide through the land the light which before beamed only from a few scattered points; if our meteor-like presence be short it is also bright; and as the meteor is remembered when the stationary lamp is unheeded, so I trust that of the tens of thousands who have felt our influence, few will forget the impression which it made on them, and fewer fail to feel that this impression ennobled and exalted their understanding. It is evident

that science now has a far more powerful hold on public opinion than when we began our course. No other proof is needed of this than the fact that many new branches of it are finding their way into the course of University instruction. Without referring to the recent changes in those of this island, I rejoice to say that in my own—that of Dublin—within the last year chemistry, thermotics, electro-magnetism, and others, have been made a portion of the under-graduate course; while one of our own valued members has introduced into primary schools a manual of zoology, of which the spirit is as good as the substance is attractive. But there is another evidence, not less satisfactory in reference to this our third object, and I name it with pleasure,—the prompt and liberal attention which our government now pays to the requests of the Association. It is true that we have never applied to it except for matters of paramount importance and unquestionable usefulness; but in times past it would have been no easy matter to force a conviction of this on the guardians of the Treasury; and we may therefore feel assured, not only that they personally take an interest in what we bring before them, but also that the whole nation sympathizes with us; for some of these concessions are of no ordinary magnitude. The completion of the Ordnance survey of Scotland—the enlarging the scale of part, perhaps all, of that of England—and the adding lines of level to that of Ireland after it was apparently completed—are very formidable items in a budget. At our demands, the observatories from which such splendid additions have been made to our knowledge of magnetism and meteorology have been established far and wide throughout our dominions:—a precious gift, not only for itself, but for what it has produced. The example was followed, on their usual princely scale, at four stations by the East India Company (always, be it said, munificent patrons of science), and still more extensively by Russia—with what success must be fresh in the memory of those who were present at the Magnetic Congress. We obtained the antarctic expedition of Ross, so fertile in its geographic fruit—so invaluable for the wide extension which it gave to the domain of terrestrial magnetism. We procured the expenditure of large sums for the reduction of the Greenwich lunar observations, and for publishing the Catalogues of Lacaille and Lalande,—and much more which I need not recite. Yet—and we well may reckon it a sign of progress—not a single voice has been raised in opposition to these grants. It seems as if our country recognized in us its scientific representatives—as if we were like the Saxon prototype of its great council: its Witena-Gemot—its assembly of the Wise.

And may we deserve that name; for let me remind you that science is not necessarily wisdom. To know, is not the sole nor even the highest office of

the intellect ; and it loses all its glory unless it act in furtherance of the great end of man's life. That end is, as both reason and revelation unite in telling us, to acquire the feelings and habits that will lead us to love and seek what is good in all its forms, and guide us by following its traces to the first Great Cause of all, where only we find it pure and unclouded. If science be cultivated in congruity with this, it is the most precious possession we can have—the most divine endowment. But if it be perverted to minister to any wicked or ignoble purpose—if it even be permitted to take too absolute a hold of the mind, or overshadow that which should be paramount over all, the perception of right, the sense of Duty—if it does not increase in us the consciousness of an Almighty and All-beneficent presence,—it lowers instead of raising us in the great scale of existence. This, however, it can never do but by our fault. All its tendencies are heavenward ; every new fact which it reveals is a ray from the origin of light, which leads us to its source. If any think otherwise, their knowledge is imperfect, or their understanding warped, or darkened by their passions. The book of nature is, like that of revelation, written by God, and therefore cannot contradict it ; both we are unable to read through all their extent, and therefore should neither wonder nor be alarmed if at times we miss the pages which reconcile any seeming inconsistency. In both, too, we may fail to interpret rightly that which is recorded ; but be assured, if we search them in quest of truth alone, each will bear witness to the other,—and physical knowledge, instead of being hostile to religion, will be found its most powerful ally, its most useful servant. Many, I know, think otherwise ; and because attempts have occasionally been made to draw from astronomy, from geology, from the modes of the growth and formation of animals and plants, arguments against the divine origin of the sacred Scripture, or even to substitute for the creative will of an intelligent first cause the blind and casual evolution of some agency of a material system, they would reject their study as fraught with danger. In this I must express my deep conviction that they do injury to that very cause which they think they are serving.

Time will not let me touch further on the cavils and errors in question ; and besides they have been often fully answered. I will only say, that I am here surrounded by many, matchless in the sciences which are supposed so dangerous, and not less conspicuous for truth and piety. If *they* find no discord between faith and knowledge, why should you or any suppose it to exist ? On the contrary, they cannot be well separated. We must know that God is, before we can confess Him ; we must know that He is wise and powerful before we can trust in Him,—that He is good before we can love Him. All these attributes, the study of His works had made known before

He gave that more perfect knowledge of himself with which we are blessed. Among the Semitic tribes his names betoken exalted nature and resistless power; among the Hellenic races they denote his wisdom; but that which we inherit from our northern ancestors denotes his goodness. All these the more perfect researches of modern science bring out in ever-increasing splendour; and I cannot conceive anything that more effectually brings home to the mind the absolute omnipresence of the Deity than high physical knowledge. I fear I have too long trespassed on your patience, yet let me point out to you a few examples. What can fill us with an overwhelming sense of His infinite wisdom like the telescope? As you sound with it the fathomless abyss of stars, till all measure of distances seems to fail and imagination alone gauges the distance; yet even there as here is the same divine harmony of forces, the same perfect conservation of systems, which the being able to trace in the pages of Newton or Laplace makes us feel as if we were more than men. If it is such a triumph of intellect to *trace* this law of the universe, how transcendent must that Greatest over all be, in which it and many like it, have their existence! That instrument tells us that the globe which we inhabit is but a speck, the existence of which cannot be perceived beyond our system. Can we then hope that in this immensity of worlds we shall not be overlooked? The microscope will answer. If the telescope lead to one verge of infinity, *it* brings us to the other; and shows us that down in the very twilight of visibility the living points which it discloses are fashioned with the most finished perfection,—that the most marvellous contrivances minister to their preservation and their enjoyment,—that as nothing is too vast for the Creator's control, so nothing is too minute or trifling for His care. At every turn the philosopher meets facts which show that man's Creator is also his Father,—things which seem to contain a special provision for his use and his happiness: but I will take only two, from their special relation to this very district. Is it possible to consider the properties which distinguish iron from other metals without a conviction that those qualities were given to it that it might be useful to man, whatever other purposes might be answered by them? That it should be ductile and plastic while influenced by heat, capable of being welded, and yet by a slight chemical change capable of adamantine hardness, —and that the metal which alone possesses properties so precious should be the most abundant of all,—must seem, as it is, a miracle of bounty. And not less marvellous is the prescient kindness which stored up in your coal-fields the exuberant vegetation of the ancient world, under circumstances which preserved this precious magazine of wealth and power, not merely till He had placed on earth beings who would use it, but even to a late period of *their* existence, lest the element that was to develope to the utmost their

civilization and energy might be wasted or abused. But I must conclude with this summary of all which I would wish to impress on your minds—that the more we know His works the nearer we are to Him. Such knowledge pleases Him; it is bright and holy, it is our purest happiness here, and will assuredly follow us into another life if rightly sought in this. May He guide us in its pursuit; and in particular, may this meeting which I have attempted to open in His name, be successful and prosperous, so that in future years they who follow me in this high office may refer to it as one to be remembered with unmixed satisfaction.

REPORTS

ON

THE STATE OF SCIENCE.

A Catalogue of Observations of Luminous Meteors; continued from the Reports of the British Association for 1848. By the Rev. BADEN POWELL, M.A., F.R.S. &c., Savilian Professor of Geometry, Oxford.

IN endeavouring to carry on the design of collecting in one record the observations of Luminous Meteors made in all parts of the world, commenced last year under the auspices of the British Association, I have received valuable aid from the communications with which I have been favoured by various correspondents; among whom I cannot omit to acknowledge my most particular obligations to Dr. Buist of Bombay, and, for by far the most extensive series of observations (both of his own, and collected from several friends), to E. J. Lowe, Esq. I have also occasionally derived other important materials from several journals. The following Catalogue, besides observations of a date subsequent to the conclusion of my former list, fills up some of its defects by observations contemporaneous with it, and others belonging to former years. I have also been enabled to prefix some notice of still earlier phenomena of this class.

In ordinary cases the original statement has been entered in the Catalogue with only slight verbal abridgements: but where there is a description of any physical appearance I have always retained the *words* of the author; and in cases where there seemed to be any peculiarity, the original document at length is given in the Appendix. The *time* is usually no more than the common clock time, unless otherwise stated: but in all Mr. Lowe's observations it is Greenwich Mean Time.

A continuation of communications is earnestly requested, addressed to the author at Oxford.

I. A valuable collection of records of Luminous Meteors and Star-showers from ancient chronicles, extending from A.D. 338 to 1223, is given by M. CHASLES in the *Comptes Rendus*, March 15, 1841.

On a comparison of the results, M. Chasles remarks the *absence* of any periodical showers in August or November. But in the earlier years there appears such a periodicity in February, and afterwards in March and April. He conjectures that the meteoric matter may form a ring, the plane of which changes continually; and thus the same matter may in later years have caused the occurrence of the November meteors.

II. For the following list of Meteorites, which have fallen in Hungary, I am indebted to W. W. SMYTH, Esq., M.A., Mining Geologist to the Geological Survey.

1559. The first accurately known as to date: 5 pieces of iron of the size of a human head, fell near Miskolez.
 1618. Three stones, each of a cwt., fell in Muraköz, of which the Turkish Pasha supplied a full account.
 1642. Between Ofen and Gran.
 1676. In Dalmatia.
 1692. Near Temeswar.
 1717 & 1740. On the Danube.
 1751. In Croatia a meteorite fell in the form of fiery lumps, and sank 18 feet in the earth.
 1808, 1812, 1814. In the Saros country. The last was a stone of 133 lbs. weight.
 1808. May. A meteorite fell at Stannern, near Blansko, in Moravia. (See Appendix, No. 1.)
 1816. Near Pest, and in Nagy Banya.
 1818. Near Mehadia, a meteor by which the whole neighbourhood was illuminated for 5 minutes.
 1820. In Oedenburg.
 1833. In the Presburg Aue.
 1833. Nov. 25. At the same place a brilliant meteor with an explosion: three meteoric stones found. (See Appendix, No. 1.)
 1834. In Zala.
 1836. At the Plattensee.
 1837 & 1842. Phænomena of the kind were seen.

III. *Observations of Meteors previous to the Date of the former Catalogue.*

Date.	Description.	Place.	Observer.	Reference.
1830. Feb. 15.....	7 ^h 10 ^m P.M. A bright meteor = full moon, moved rapidly from N.E. to S.W., after about 4 seconds vanished, leaving a train of light slightly wavering.	Near Birmingham.	Dr. Hopkins...	Appendix, No. 15.
1838.	7 ^h 30 ^m P.M. A bright meteor = full moon, <i>but of singularly contorted form</i> —perfectly stationary for about 20 minutes, then gradually disappeared.	Palamecottah, South India	Rev. G. Pettit..	Appendix, No. 16.

Observations of Meteors in 1843 and 1844, communicated by Dr. Buist.
See Appendix, No. 3.

From the unpublished Reports of the Colaba Magnetic Observatory.

Date.	Bombay Mean Time.	Description.	Magnitude.	Duration.
1843.	h m			sec.
Nov. 8.....	3 42 a.m....	A meteor passed from S.W. to S.		
9.....	11 39 p.m....	Do. from a little above the horizon to- wards W.		
14.....	0 52 a.m....	Do. northward from below the zenith		
	1 37 a.m....	Do. from little above the S. horizon to the S. horizon.		
	3 28 a.m....	Do. by the zenith from N. to S. Do. far below zenith from the N. to S. horizon.		
	8 35 p.m....	Do. a little below zenith.		
	8 41 p.m....	Do. another in the same direction.		
	8 46 p.m....	Do. another a little below zenith in the N.W.		
	8 49 p.m....	Do. another in the zenith.		
	11 27 p.m....	Do. a little below zenith towards S.W.		
	11 55 p.m....	Do. a little above the horizon in the W.		
15.....	1 45 30 ^a a.m....	Do. from α Argus to the S.W.		
	1 57 a.m....	Do. from Sirius to the E.		
	10 22 p.m....	Do. from far below zenith to the S.		
	10 26 p.m....	Do. from a little above the horizon to the E.		
	10 33 p.m....	Do. from a little above the horizon to the E.		
	10 45 p.m....	Do. from a little above the horizon to the S.		
	10 55 p.m....	Do. from a little above the horizon to the N.		
16.....	0 24 a.m....	Do. from Orion towards the E.		
	6 37 a.m....	Do. from a little above the horizon to N.E.		
	10 58 p.m....	Do. from a little above the horizon to- wards E.		
	11 22 p.m....	Do. from a little above the horizon to- wards N.		
	11 37 p.m....	Do. from a little below zenith to S.E.		
	11 52 p.m....	Do. from far below zenith to S.		
17.....	8 24 p.m....	Do. in the N.		
	10 7 p.m....	Do. from a little below zenith towards S.		
20.....	8 33 p.m....	Do. from N.E. to N.		
	10 3 p.m....	Do. from S. to S.E.		
1844.				
Nov. 2.....	2 49 a.m....	Brilliant meteor from about 15° above the S. horizon to it.	1	4
3.....	3 52 a.m....	Small do. to the western horizon from a little above it.	5	
	4 41 a.m....	Do. by Ursa Major towards eastern horizon.		
4.....	2 47 a.m....	Do. westward from the zenith.....	6	
	3 39 a.m....	Meteor passed to the S. horizon from a little above it.	2	
	3 45 a.m....	Brilliant do. to the western horizon from a little above it.	1	

Date.	Bombay Mean Time.	Description.	Magnitude.	Duration. sec.
1844.	h m			
Nov. 4.....	7 46 p.m....	Meteor passed in a western direction from above the northern horizon.	4	
5.....	9 35 p.m....	Do. in the N.E. towards the N.E. horizon.	4	
6.....	7 56 p.m....	Do. from the zenith towards the S.....	4	
7.....	2 20 a.m....	Do. southward	2	
	2 32 a.m....	Brilliant do. northward, from the zenith.	1	
	2 57 a.m....	Small do. passed westward	6	
	2 57 30 a.m.	Meteor southward from the zenith	3	
	3 45 a.m....	Very brilliant do. rapidly from the E. of zenith to the S.	1	3
	4 46 a.m....	Meteor eastward from a little above the N. horizon.		
	10 49 p.m....	Do. in the north, passed towards the N. horizon.	3	
	10 57 p.m....	Do. in the N.E. of the 3rd magnitude. Two others in the constellation Orion, one of the 5th, the other of the 3rd magnitude; the latter a shooting one.		
	11 22 p.m....	Do. in the zenith of the 6th magnitude; another near Gemini, in the N.E. at 11 ^h 51 ^m P.M., of the 3rd magnitude.		
8.....	2 56 a.m....	Small do. from the Pleiades to the N....	6	
	4 22 a.m....	Meteor from the zenith to the S.....	3	
	4 24 a.m....	Brilliant do. westward from a little above the Pole-star.	1	3
	11 41 p.m....	Meteor in the N.E.	4	
	11 55 p.m....	Do. in the N.E.....	5	
9.....	0 23 a.m....	Do. northward from the zenith	3	
	1 27 a.m....	Do. to the southern horizon from a little above it.	3	
	1 40 a.m....	Small do. to the W. horizon from a little above it.	5	
	2 22 a.m....	Brilliant do. from the west of α Orionis to S.W.	1	2
	2 41 a.m....	Three meteors passed from the north of the zenith, one to the west, one to the south, and one to the north.	6	
	2 47 a.m....	Four or five meteors were observed going to and fro in the zenith.	6	
	3 27 a.m....	Meteor rapidly from far below the zenith to N.E.	1	
	3 42 a.m....	Do. followed by another smaller one, eastward from zenith.	1 & 6	
	3 47 a.m....	Very brilliant do. from Ursa Major to the north.	1	1
	3 59 a.m....	Meteor from a little above the W. horizon to N.W.	2	
	4 46 a.m....	Do. from above the horizon to west.....	4	
	7 40 p.m....	Do. from a little below the zenith to S.	3	
	7 57 p.m....	Do. from a little above the North Pole-star towards the horizon.		
	8 27 p.m....	Very small do. from N. towards W.....	5	
	8 38 p.m....	Small do. from a little below zenith towards N.	4	
	9 27 p.m....	Meteor from the zenith towards W. ...	4	
	9 32 p.m....	Do. from N. to N.E.....	3	
10.....	1 20 a.m....	Do. eastward from the zenith	3	

Date.	Bombay Mean Time.	Description.	Magnitude.	Duration.
1844.	h m			sec.
Nov. 10.....	1 25 a.m....	Small meteor westward from a little below the zenith.	5	
	1 39 a.m....	Do. N.E. from Ursa Major	6	
	1 47 a.m....	Meteor by the E. of Ursa Major to E. horizon.	2	
	2 50 a.m....	Do. from a little below the S. horizon to the E.	2	
	2 57 a.m....	Brilliant do. E. from Sirius, to Milky Way.	1	1
	3 26 a.m....	Do. by the E. of Ursa Major to the N.E. horizon.	1	
	3 39 a.m....	Meteor rapidly by the W. of Pole-star to the N.	1	
	3 45 a.m....	Small do. westward from the zenith ...	6	1
	3 46 a.m....	Do. towards the N.	6	
	3 55 a.m....	Meteor near the zenith, and was lost near Ursa Major.	3	1½
	4 42 a.m....	Do. from below the zenith, passed towards the northern horizon.	4	
	7 50 p.m....	A small meteor appeared in the north, very near the zenith, where it was lost.	4	
	11 25 p.m....	Two meteors flashed along the zenith...	5 & 6	
	11 36 p.m....	Meteor in the east, passed southward...	5	
	11 53 p.m....	Do. in the N.E., passed towards the horizon.	5	
11.....	1 37 a.m....	Two small meteors were observed in the Milky Way, one passing to the E. the other to the W.	6	
	1 42 a.m....	Meteor near α Orionis, passed eastward from the Milky Way.	3	
	1 51 a.m....	Brilliant do. from a little above the Pole-star to the N. horizon.	1	
	2 3 a.m....	Small do. eastward from a little below the zenith.	5	
	2 37 a.m....	Three small meteors, at the interval of minutes, were seen in the Milky Way, E. of zenith.	6	
	2 47 a.m....	Meteor passed rapidly from the E. of the zenith to it.	3	
	2 48 a.m....	Brilliant do. from Aldebaran towards the W.	1	
	3 20 a.m....	Do. northward from η Ursa Major	1	
	3 38 a.m....	Meteor northward from a little above the Pole-star.	1	
	4 34 a.m....	Do. from the zenith towards the E., and another from below the zenith towards the eastern horizon.		
	7 52 p.m....	Two do. successively, one from above the horizon westward, and another towards the S.	3 & 4	
	10 44 p.m....	Small do. in the N.E. by E., a little below the zenith.	5	
	10 56 p.m....	Do. in the N.E., a little below the zenith.	5	
	11 29 p.m....	Meteor in the N.E., a little below the zenith.	4	
12.....	1 27 a.m....	Brilliant do. from Canis Minor to the S.W.	1	

Date.	Bombay Mean Time.	Description.	Magnitude.	Duration.
1844.	h m			sec.
Nov. 12.....	1 42 a.m....	Very brilliant meteor from a little above Ursa Major to the W.	1	1
	1 57 a.m....	Meteor from Cassiopeia to the W.	3	
	2 2 a.m....	Small do. near Cassiopeia.....	5	
	2 20 a.m....	Brilliant do. from the E. of Ursa Major to N.E.	1	$\frac{1}{2}$
	2 20 30 ^a .m.	Smaller one from a little below α Eridani to the S. horizon.	3	
	2 26 a.m....	Small do. from β Arietis to the W. ...	2	
	2 37 a.m....	Meteor from β Cassiopeia to the N. ...	2	
	2 46 a.m....	Do. westward from σ Arietis	2	
	2 49 a.m....	Brilliant do. a little below Canis Minor, and was lost near Castor.		
	3 00 a.m....	Meteor appeared near Auriga, and was lost near Pollux.	1	
	3 30 a.m....	Do. southward from the E. of Canis Major.	1	
	3 45 a.m....	Small do. in the Milky Way, a little above the S. horizon.	5	
	3 49 a.m....	Meteor towards the E. of zenith.....	5	
	3 55 a.m....	Do. southward from Canis Major	1	
	3 59 a.m....	Do. in the direction of N. to S. in the E.	1	
	4 20 a.m....	Do. from E. towards Ursa Major	3	
	7 56 p.m....	Do. from below the zenith to the S. horizon.	3	
	10 53 p.m....	Small do. from a little below the zenith towards the horizon.	6	
	11 17 p.m....	Do. in the N.E.....	7	
13.....	0 22 p.m....	Brilliant do. from the zenith towards W.	2	
	0 35 a.m....	Meteor from a little below the zenith, eastward.	1	1
	0 48 a.m....	Do. from below the zenith to E.....	3	$\frac{1}{2}$
	0 55 a.m....	Brilliant do. from below the zenith towards W.	3	1
	1 32 a.m....	Meteor from a little below the zenith to the N.	3	
	1 45 a.m....	Do. along Aldebaran towards the E. horizon.	2	
	1 50 a.m....	Do. from Ursa Major to the N. horizon..	3	
	2 00 a.m....	Do. in the N. towards the horizon	2	
	2 25 a.m....	Do. from Aldebaran to the E.	4	
	2 47 a.m....	Do. in the N. passed towards Ursa Major.	3	
	2 55 a.m....	Do. from Aldebaran to E.	3	
	3 25 a.m....	Do. from the Pleiades towards W.	3	
	3 33 a.m....	Do. in the N. passed towards the Pleiades in a W. direction.	4	
	3 54 a.m....	Three do. successively in the N.; one along Aquila; one towards Ursa Major, and one towards the horizon.	1, 2 & 3	
	7 55 p.m....	Meteor towards the eastern horizon ...	2	
14.....	1 28 a.m....	Small do. from α Ursa Major to the N.	6	
	1 40 a.m....	Meteor between Aldebaran and Orion..	2	
	1 57 a.m....	Do. to α Ursa Major from a little above it.	2	
	2 55 a.m....	Do. southward in the Milky Way	5	
	2 56 a.m....	Do. in the Milky Way.....	4	

Date.	Bombay Mean Time.	Description.	Magnitude.	Duration.
1844.	h m			sec.
Nov. 14	2 57 a.m.—	Meteor to α Ursæ Majoris from a little above it.	1	
	3 25 a.m....	Very brilliant do. westward from α Ursæ Majoris. Its trail of light was visible for a second.	1	2½
	3 27 a.m....	Small do. westward by the S. side of Cassiopeia.	6	
	3 46 a.m....	Brilliant do. near the Milky Way, and was lost near α Eridani.	1	
	3 47 a.m....	Two smaller meteors appeared in the western side of the Milky Way, and were lost near the S. horizon.	3	
	3 48 a.m....	Brilliant meteor passed southward from the E.	1	
	4 42 a.m....	Meteor in the E. towards the horizon.	2	
15	0 17 a.m....	Do. in the S.		
	0 57 a.m....	Do. in the W.	5	
	1 32 a.m....	Do. from a little below Castor to the S.	3	
	1 41 a.m....	Brilliant do. from a little above the S.W. horizon to it.	1	
	1 46 a.m....	Small do. southward from η Orionis ...	6	
	1 57 a.m....	Very brilliant do. from the E. to the N.	1	2
	3 19 a.m....	Brilliant do. from a little above the S. horizon to it.	1	2
	3 31 a.m....	Meteor westward from α Arietis.....	1	2
	3 45 a.m....	Very brilliant do. from a little below α Aurigæ towards the S.	1	1½
	4 42 a.m....	Small do. eastward from Ursa Major...	3	
16	3 25 a.m....	Meteor to the N.W. from a little below Canis Major.	2	
	3 32 a.m....	Do. to the N. horizon from β Ursæ Minoris.	3	
	3 46 a.m....	Do. northward from β Ursa Major.....	1	
	3 47 a.m....	Two do. to the eastern horizon from the northern side of the Planet Venus.	1	
	4 30 a.m....	Meteor westward from the E.....	3	
	4 45 a.m....	Do. from below the zenith to the E. ...	3	
	8 10 p.m....	Do. from the E. towards the S.E. horizon	3	
	8 13 p.m....	Do. from the N.E. towards the N. from Aldebaran.	3	
17	2 38 a.m....	Do. eastward from Canis Minor	1	
	2 57 a.m....	Do. to the southern horizon from the eastern side of α Eridani.	3	
	4 17 a.m....	Do. down perpendicularly from Orion...	1	1
	4 30 a.m....	Do. near the zenith	3	
	4 42 a.m....	Do. from a little below the zenith to E.	2	1
	5 03 a.m....	Do. from Venus toward the E. horizon..	1	
	9 04 p.m....	Brilliant do. westward from Cassiopeia..	1	2
18	2 27 a.m....	Meteor from a little below the zenith towards the S.	2	½
	2 47 a.m....	Do. in the S. towards Canis Major.....	2	½
	2 55 a.m....	Brilliant do. from a little above Canis Major towards the S. horizon.	1	½
	3 47 a.m....	Meteor in the S. dropped down towards the horizon.	2	¼
19	2 35 a.m....	Very brilliant do. rapidly from a little above it to the N.E. horizon.	1	
	2 51 a.m....	Meteor a little above Ursa Major	3	

Date.	Bombay Mean Time.	Description.	Magnitude.	Duration.
1844.	h m			sec.
Nov. 19.....	2 57 a.m....	Meteor eastward from a little below Canis Minor, leaving sparks behind during its progress for half a second.	2	
	3 45 a.m....	Small do. descended from the zenith westward.	5	
	4 02 a.m....	Meteor westward from α Eridani	3	
20.....	2 21 a.m....	Do. in the E.	1	
	4 04 a.m....	Do. in the S.....	2	
	7 57 p.m....	Brilliant do. from the Pleiades towards the zenith.	1	
	10 00 p.m....	Do. from α Orionis to the S., with a little inclination to the W.	1	
21.....	1 30 a.m....	Large do. descended to the E. horizon from a little above it.	1	3
22.....	3 42 a.m....	Meteor to α Ursa Minor from the N.W.	2	
	4 57 a.m....	Do. from Ursa Major towards the E....	2	
	5 3 a.m....	Do. from the Pleiades towards the W. horizon.	3	
23.....	3 52 a.m....	Do. in the N. near Ursa Minor.		
24.....	4 34 a.m....	Do. from the S. towards the Pleiades...	2	
	4 47 a.m....	Do. from the S. horizon towards Venus..	2	
	4 55 a.m....	Do. from a little below the zenith to- wards the E.	2	
26.....	4 48 a.m....	Do. in the S.		
27.....	7 00 p.m....	Do. in the W. towards the horizon.....	1	1

IV. *Supplement to the Table in last Report of the British Association, from August 1846 to July 1848.*

Date.	Description.	Place.	Observer.	Reference.
1846.				
Aug. 8, 9.....	Night cloudy	Newhaven..	Mr. Herrick...	Bulletin*, Acad. R. Bruxelles, 1847. 201.
10	Clear interval about midnight; 15 meteors from 12 ^h to 1 ^h ; 32 do. from 1 ^h to 2 ^h .	Connecticut }		
11	Clear, moonlight; 41 meteors from 9 ^h to 10 ^h 41 ^m ; 18 in N.W. quadrant; 23 in S.E.; faint aurora.	Ibid		
Sept. (Middle)	Numerous shooting stars	Illyria and Dalmatia.	M. Colla	Ibid. 139.
Nov. 8	Many	Milan	M. Mayer.....	Ibid. 43.
8, 9 ...	Many; brilliant	Parma	M. Colla	Ibid.
11	Fall of an aërolite	Lowell, U.S....	Correspondent to M. Colla.	Ibid.
12, 13, 14, 15	Many; brilliant; most N.E. and S.W.	Parma	M. Colla	Ibid.

* Some of the meteors noticed in the Number of the Bulletin here referred to, are the same as those recorded in the former Catalogue, and are therefore not here inserted,

Date.	Description.	Place.	Observer.	Reference.
1846.				
Nov. 13	27 between 7 ^h 11 ^m and 7 ^h 53 ^m .	Milan	M. Mayer.....	Bull. Acad. R. Brux., 1847. 43.
12, 13...	During both nights 239 meteors.	Vienna	Correspondent to M. Mayer	Ibid.
Dec. 9	Many shooting stars	Parma	M. Colla	Ibid.
10	Many do.....	Ibid	Id.	Ibid.
21	A remarkable bolide (morning)	Ibid	Id.	Ibid.
1847.				
Jan. 10	Many shooting stars	Parma	M. Colla	Ibid. 268.
Mar. 11, 12, 24, 25	Many. On the 12th a faint aurora.	Ibid.....	Id.	Ibid.
April 19, 20...	Many	Ibid.....	Id.	Ibid.
May 26	10 ^h 25 ^m P.M. A large meteor; bluish white. Altitude about 12° or 15° in S.E.; descended towards N.E.	Rosehill, Oxford.	Rev. J. Slatter...	Letter to Professor Powell.
June 17-22...	Many meteors.....	Parma	M. Colla	Bull. Acad. R. Brux., 1847, 268.
29	9 ^h 48 ^m P.M.; altitude 52°, azim. 50° W. A yellow light over the sky; then from behind clouds a globe of fire descended slowly in direction of meridian, through 20° towards S.; disappeared without noise. Mostly hid by clouds; size could not be estimated, but seemed large.	Ibid.....	Id.	Ibid.
July 4, 5 ...	A great number of meteors ...	Ibid.....	Id.	Ibid. 406.
14	3 ^h 45 ^m A.M. Violent explosions and a rushing noise; then a narrow black cloud emitting coruscations; divided and disappeared. A meteorite seen to fall; imbedded itself 3 feet deep in the ground. Another mass penetrated and shattered a house.	Hauptmannsdorf, near and N.E. of Braunau, Bohemia.	Correspondent to W.W. Smyth, Esq.	Appendix, No. 2.
14	Large meteorite dug up, 14 feet deep, at See-Læggen.	Bohemia	Correspondent to Prof. Boguslawski.	British Assoc. 1848. Athenæum, No. 1087, p. 866.
22, 23..	A great number of meteors ...	Parma	M. Colla	Bull. Acad. R. Brux., 1847. 406.
Aug. 9	Early in the evening, 10 meteors in an hour; towards midnight, 20 meteors in an hour.	Bruxelles Observatory.	M. Quetelet.....	Ibid. 382.
	One remarkable at 10 ^h 25 ^m , appeared in Pegasus; slow; visible for 5 seconds; disappeared in Ophiuchus; no defined nucleus, but nebulous head. Expanded to about 30' diam., and so dissipated.	Ibid.....	Id.	Ibid. 383.
	Nine meteors from 9 ^h 25 ^m to 10 ^h 20 ^m with trains.	Ghent	M. Duprez	Ibid.
10	Cloudy at Brussels. From 9 ^h 20 ^m to 9 ^h 55 ^m . 28 meteors.	Aix la Chapelle	M. Heis	Ibid. 385.

Date.	Description.	Place.	Observer.	Reference.
1847.				
Aug. 10	Some with trains; mostly in Via Lactea; one moving <i>upwards</i> ; 20 in $\frac{1}{4}$ hour.	Munich	M. Robiano	Bull. Acad. R. Brux., 1847. 386.
11	Great numbers; 30 in an hour; mostly from N.E. to S.W.	Brussels	M. Quetelet.....	Ibid. 383.
	From 9 ^h 15 ^m to 12 ^h 15 ^m 66 meteors; 27 brilliant, with trains looking towards E., or 22 per hour (in former years 21 per hour). 27 from N.E. to S.W.; the rest in different directions.	Ghent	M. Duprez	Ibid.
	35 in 1 hour; most towards W.; many red; some with trains.	Bruges.....	Dr. Forster	Ibid. 384.
	From 9 ^h to 13 ^h 30 ^m 501; number greater as night advanced. Emanating from a point; R 42°; dec. 55° S.	Aix la Chapelle	M. Heis	Ibid. 385.
7-13, 16, 17, 23, 24	} Great number of meteors.	Parma	M. Colla	Ibid. 406.
Sept. 7		Bombay	A Correspondent	Bombay Times, Nov. 1, 1847. Appendix, No. 4.
Oct. 10	100 in one hour; maximum about 2 A.M., Oct. 11.	Bruges.....	Dr. Forster	Bull. Acad. R. Brux., 1847. 404.
	Great numbers of meteors	Parma	M. Colla	Ibid. 406.
30	Large fire-ball; nearly horizontal from E. to W.; then fell perpendicularly into the sea. Very bright; blue; train.	Bombay	A Correspondent	Bombay Times, Nov. 7, 1847. Appendix, No. 4.
Nov. 13	Cloudy; clear interval from 10 ^h to 13 ^h 58 ^m . Several hundreds of small meteors, white, with trains; mostly N.N.W. One large; slow; across zenith to S.E.	Bruges.....	Dr. Forster	Astr. Soc. Notices, ix. 37.
Dec. 20	Three meteors.....	Ibid.....	Id.	Ibid.
1848.				
Jan. 27	3 ^h P.M. (daylight). Clear sky. A bright meteor passed from S.W. to N.E., between 60° and 30° alt.; pear-shaped; of a silvery whiteness, with a train which separated into several parts. Visible 3 secs.	Buckingham, and at one mile distant.	The brother of Rev. J. Slatter, and another Observer.	MS. letter from Rev. J. Slatter to Prof. Powell.
Feb. 15	1 ^h P.M. A loud report and rushing noise; nothing seen to fall, but dust rising. Meteorite found several inches deep.	Negloor, near Dharwar, India.	Captain S. Wingate, Bombay Engineers.	Bombay Times. Appendix, No. 5.
March 8	4 ^h A.M. A large meteor, shaped like a kite, larger and brighter than the moon, passed slowly and nearly horizontally from W. to E. in the S. It appeared to issue from behind the clouds, and was lost behind them again. Seen also at Bath.	Slough.....	Mr. Atkins, Correspondent to Sir J. Herschel.	MS. letter. Appendix, No. 6.

Date.	Description.	Place.	Observer.	Reference.
1848.				
April 6	Soon after sunset. (24 just visible). A small meteor; very white; moved from S.E. towards S.W., and then fell nearly perpendicularly.	Near Rosehill, about two miles S.E. of Oxford.	Rev. J. Slatter...	MS. letter to Prof. Powell.
12	9 ^h 15 ^m P.M. A meteor about the size and colour of Antares; moved slowly nearly horizontally along E.; descended a little below α Lyrae, and then disappeared.	Oxford	Rev. J. Slatter...	Ibid.
18	After sunset, hardly dark. A meteor in S.S.E. appeared to ascend a little, and then descended almost perpendicularly, increasing in brightness till it disappeared.	Between Oxford and Rosehill.	Rev. J. Slatter...	Ibid.
July 13	10 ^h P.M. A bright meteor from S. to W. at about 35° alt.; head "the size of a full-sized cricket-ball;" cream colour; middle of train purplish red, hind part bluish green.	Stone Easton, 14 miles S.W. of Bath.	H. Lawson, Esq., F.R.S. &c.	MS. letter to Mr. Lowe.
15	11 ^h P.M. A meteor=star 1st mag.; fell perpendicularly from 24 Camelopardalis (Hevelius) through (B.C.) 43 and 42 Camelopardalis, and through (B.C.) 14 Lyrae.	Highfield House, Nottingham.	E. J. Lowe, Esq.	MS. list communicated to Prof. Powell.

NOTE.—Thus far the present Catalogue coincides in time with the latter part of the former. On comparing them we may remark—

1846. Aug. 8, 9, 10.—The observations at Newhaven, U.S., and at Dijon, agree in showing numerous meteors.

Dec. 21.—May the large meteor observed at Nottingham at 9 P.M. be connected with the bolide at Dijon "in the morning?"

1847. Jan. 10, 11.—Many meteors were observed both at Nottingham and Parma.

June 21.—Several at Nottingham. 17–22. Many at Parma.

Aug. 9, 10, 11.—Observations agree at Nottingham, Durham, Oxford, Belgium, and Aix la Chapelle.

Nov. 12, 13.—Observations agree at Durham, Bruges, and Benares.

1848. April 6.—The same meteor seen by Mr. Symonds and Mr. Slatter.

V. The Table in Report 1848, continued.

Date.	Description.	Place.	Observer.	Reference.
1848.				
Aug. 1	10 ^h 25 ^m . Large and brilliant; from near 37 (Hevelius) Ursæ Majoris, through λ Urs. Maj. to 21 (B.C.) Leonis Minoris; with a train.	Highfield House, Nottingham.	E. J. Lowe, Esq.	MS. communication to Prof. Powell.
	10 ^h 30 ^m to 10 ^h 50 ^m . Several small, with trains, in Andromeda, Pegasus, and Cassiopeia.	Ibid.....	Id.	Ibid.

Date.	Description.	Place.	Observer.	Reference.
1848.				
Aug. 1	10 ^h 51 ^m . Small, with train, from α Androm. to α Pegasi.	Highfield Ho., Nottingham.	E. J. Lowe, Esq.	MS. com. to Prof. Powell.
	11 ^h 7 ^m 30 ^s . Small, extremely rapid; parallel to horizon from tail to head of Vulpecula, with streamers.	Ibid.....	Id.	Ibid.
	11 ^h 8 ^m . Small, with streamers; from 10° below Delphinus through δ Antin.; rapid.	Ibid.....	Id.	Ibid.
	11 ^h 12 ^m . Small, with streamers, perpendicularly down from δ Sagittarii.	Ibid.....	Id.	Ibid.
	11 ^h 15 ^m . Bright; red; with train, inclined 45° towards W. horizon; first seen near τ Urs. Maj.; fell to λ Urs. Maj.	Ibid.....	Id.	Ibid.
	11 ^h 41 ^m . Small; from α Delphini towards S.; vanished after moving 1½°.	Ibid.....	Id.	Ibid.
	11 ^h 42 ^m . Small; moved vertically upwards from α Delphini to hind leg of Wolf.	Ibid.....	Id.	Ibid.
2	10 ^h 44 ^m . Brilliant; path inclined at about 45° from α Androm. to Triangulum.	Ibid.....	Mrs. Lowe	Ibid.
	10 ^h 49 ^m . Parallel to horizon, from ϵ to τ Cygni.	Ibid.....	Id.	Ibid.
7	11 ^h 19 ^m . Slow, from S. towards E. at about 45°; first seen at η Antin. and fell slightly to E. of β Capricorni.	Ibid.....	E. J. Lowe, Esq.	Ibid.
9	From 9 ^h 45 ^m P.M. to 11. 25 meteors; from 12 ^h to 1 ^h 30 ^m 70. Mostly from N. to S.	Paris?	M. Goujon	Comptes Rendus, 1848. ii. 185.
	A list of the number seen on different nights, during a part of July and August; showing a maximum on August 9, 10.	Ibid.....	M. Coulvier Gravier.	Ibid.
	During the night of 9, 10, 414 seen.			
	9 ^h P.M. A remarkable number of shooting stars, apparently lower in the atmosphere than usual; of a dark red colour; direction mostly S.E.	Near Hampton.	Sir R. H. Schomburgk.	Athenaeum, No. 1085, p. 807.
	[No meteors on the 8th.]	St. Leonards, Sussex.	Prof. Powell.	
	9 ^h P.M. to 9 ^h 30 ^m . Six small meteors, with trains, following in parallel directions from S.E. to S.; inclined at about 30° to S. horizon.			
	Flashes of lightning in S.			
	9 ^h 30 ^m . A brilliant flash, or coruscation, nearly at S. point of horizon.			
	9 ^h 35 ^m . A bright meteor from S.E. to S. (as before); another from zenith; nearly parallel.			

Date.	Description.	Place.	Observer.	Reference.
1848.				
Aug. 9	9 ^h 50 ^m . Small meteor from S.E. to S. (as before); another from zenith; nearly parallel.	St. Leonards, Sussex.	Prof. Powell.	
	10 ^m . Clouded over			
	From 9 ^h 3 ^h to 11 ^h 1 ^h . 18 meteors; most from N.W. to S.E.; one N.E. to S.W. (moonlight).	Ghent	M. Duprez	L'Institut, No. 783, p. 6.
	11 ^h 19 ^m . One large; appeared near head of Medusa; moved E.; red; train of sparks.	Ibid.....	Id.	Ibid.
	Before 11 ^h P.M. Several small falling stars.	Highfield Ho., Nottingham	E. J. Lowe, Esq.	MS. com. to Prof. Powell.
	11 ^h 27 ^m . One; blue; rapidly from 3° below α Androm.; parallel to horizon to ϵ Pegasi.	Ibid.....	Id.	Ibid.
	11 ^h 29 ^m . Red; shot downwards at 25° incl. from β to λ Pegasi.	Ibid.....	Id.	Ibid.
	11 ^h 34 ^m . Bright; fell rapidly from 35 (Hevel.) Cassiopeia, through 17 Camelopardalis.	Ibid.....	Id.	Ibid.
	11 ^h 37 ^m . One through β Pegasi to Antinous, "in a curious curve, which was inverted with respect to the horizon, towards which it was falling," (probably convex to the horizon?)	Ibid.....	Id.	Ibid.
	11 ^h 52 ^m . One from λ to α Draconis; streamers.	Ibid.....	Id.	Ibid.
	11 ^h 54 ^m . Two together; one from χ Cephei towards γ Urs. Min., the other from λ to η Draconis; streamers.	Ibid.....	Id.	Ibid.
	11 ^h 57 ^m . One from 4° above Polaris through η Draconis; many smaller during the evening, with tails.	Ibid.....	Id.	Ibid.
	11 ^h 58 ^m . Bright; from χ Cephei to Lacerta.	Ibid.....	Id.	Ibid.
	12 ^h 4 ^m . Two together, perpendicularly downwards; one slightly W. of α , and through β Urs. Maj.; the other 1° E. of δ , and through γ Urs. Maj.	Ibid.....	Id.	Ibid.
	12 ^h 5 ^m . Two, both perpendicularly downwards; 1st through γ Urs. Maj.; 2nd from χ Cephei. Many smaller.	Ibid.....	Id.	Ibid.
10	11 ^h 41 ^m P.M. One; very rapid; from Shedii to γ Cephei.	Ibid.....	Id.	Ibid.
	11 ^h 44 ^m . One; very rapid; from α Cephei through γ Draconis and η Herculis.	Ibid.....	Id.	Ibid.
	11 ^h 50 ^m . One; small; 5° under Polaris; rapid; horizontal. Many small ones all night, especially in Cepheus, Ursa Minor and Draco.	Ibid.....	Id.	Ibid.

Date.	Description.	Place.	Observer.	Reference.
1848.				
Aug. 10	Cloudy till 11 $\frac{3}{4}$, thence to 12 $\frac{3}{4}$; 9 meteors; most from N. to S.; then cloudy. Partially cloudy, from 12 ^h to 12 ^h 45 ^m , 30 seen through clouds; bluish white, with red train. Clearer from 1 ^h 41 ^m to 2 ^h 41 ^m ; 117 seen; perhaps smaller ones not noticed. Some large and brilliant; motion mostly directed towards η Ophiuchi, nearly $\frac{5}{8}$ N. of the prime vertical. No meteors before 9 ^h 30 ^m P.M., then partially clouded. No meteors seen.	Ghent	M. Duprez	L'Institut. No. 783, p. 6.
		Aimagh	Rev. Dr. Robinson and Assistant.	British Association, 1848. Sec. Proc., p. 37.
		St. Leonards, Sussex.	Prof. Powell.	
11	Entirely clouded.....	Ibid.....	Id.	Ibid.
21	11 ^h 45 ^m . A brilliant caudate meteor, larger than Sirius, from Delphinus to Algenib. 11 ^h 47 ^m . A small one from γ Urs. Min., perpendicularly downwards. Many smaller ones about midnight.	Highfield House, Nottingham.	E. J. Lowe, Esq.	MS. communication to Prof. Powell.
		Ibid.....	Id.	Ibid.
23	11 ^h 40 ^m . Bright; perpendicularly down from 1° N. of γ Urs. Maj. Several small ones within the last few minutes in Draco, Andromeda, Pegasus, and Ursa Minor.	Ibid.....	Id.	Ibid.
		Ibid.....	Id.	Ibid.
28	10 ^h . Small meteors in Ursa Minor and Draco.	Ibid.....	Id.	Ibid.
29	Many in Urs. Maj. and Min. ... A meteor (no particulars given).	Ibid.....	Id.	Ibid.
		Paris	M. Dubois	Comptes Rendus, 1848. ii. 297.
Sept. 1	7 ^h 45 ^m . A meteor (no particulars given). 8 ^h P.M. Brilliant meteor; greenish light; slow; nearly horizontal, from W. to E. "Bright globe of fire," from N.W. to N.E.; sky illuminated after disappearance. 9 ^h the same?	Saffres Côté d'Or. Brussels	M. Boucher..... M. Putzeys	Ibid. L'Institut, No. 783, p. 6.
		Nevers and Caen.	Correspondent to Nevers Journal.	Ibid.
4	8 ^h 59 ^m P.M. A meteor from about η Antinoi to π Sagittarii, = 6 times γ ; dark straw-colour, changing to purple; emitted sparks; faded away, leaving a streak of blue light 4° in length, and 25' in breadth, perpendicular to horizon; lasted $\frac{3}{4}$ min. before it finally vanished; a small falling star crossed in same track.	Highfield House, Nottingham.	E. J. Lowe, Esq. and A. S. H. Lowe, Esq.	MS. communication to Prof. Powell.

Date.	Description.	Place.	Observer.	Reference.
1848.				
Sept. 4	9 ^h P.M. In the S.S.W. a meteor "like a sky-rocket, or rather an oblong piece of fire," first blue, then fiery red, emitting sparks, continued for 1 or 2 seconds, and disappeared, leaving a blue mark visible for many seconds, from Altair perpendicularly down. Seen also at Pecamp, in France, like a globe of fire emitting sparks.	Worthing, Sussex.	Alfred H. Lowe, Esq.	Ibid.
	Soon after a small falling star moved in the same path.	Ibid.....	Id.	Ibid.
6	12 ^h 30 ^m . A few falling stars ...	Highfield Ho., [Nottingham]	E. J. Lowe, Esq.	MS. commun.
8	6 ^h 50 ^m P.M. A small luminous ball from N.W. to S.E.; dissipated 'before reaching the horizon.	Pisa	J. Irving, Esq....	Letter to Prof. Powell, in Appendix, No. 7.
19	12 ^h . One from χ Draconis to midway between δ and ϵ Urs. Maj.	Highfield Ho., Nottingham	E. J. Lowe, Esq.	MS. Communication.
24	11 ^h 18 ^m . A caudate meteor = 1st mag.; slowly down from 3° E. of γ Urs. Maj. to ψ Urs. Maj.	Ibid.....	Id.	Ibid.
Oct. 5	11 ^h 27 ^m P.M. Small meteor, from α Pegasi through Cassiopeia.	Ibid.....	Id.	Ibid.
	11 ^h 33 ^m . Do. with many red sparks; from 5° below Capella; moved 1° and disappeared.	Ibid.....	Id.	Ibid.
18	10 ^h 46 ^m . During a magnificent aurora, a fine caudate meteor fell through Pegasus Square. Several falling stars.	Ibid.....	Id.	Ibid.
19	8 ^h . One, from 1° below η Herculis, horizontally to 1° above ϵ Herculis.	Ibid.....	Id.	Ibid.
21	11 ^h 11 ^m . One from between the Pointers to γ Urs. Maj.	Ibid.....	Id.	Ibid.
22	Many; one at 8 ^h 50 ^m = 1st mag. fell through the Wagon in Urs. Maj.	Ibid.....	Id.	Ibid.
27	10 ^h 30 ^m . Large and brilliant, through Aldebaran down to Betelgeuse. Many others.	Ibid.....	Id.	Ibid.
	8 ^h P.M. Large and white, with a train; = Venus; passed through Taurus in a line parallel to Aldebaran, and the two adjacent stars, and disappeared at a distance beyond Aldebaran, nearly = that of Aldebaran from the next star; at the same time a bright aurora*.	Oxford	Mr. G. A. Rowell.	Verbal Statement to Prof. Powell.

NOTE.—1848, Sept. 4.—The meteor seen at Nottingham was probably the same with that seen at Worthing and Pecamp; the stars η Antinoi, Altair (α Aquilæ), and π Sagittarii, all lying not much out of the same line, passing down to the horizon at the time.

* See also Phil. Mag. Dec. 1848, calculation by Sir J. Lubbock, and Phil. Mag. March 1849.

Date.	Description.			Time of Light.	Remarks.	Place.	Observer.	Reference.
	Aden. Mean Time.	Magnitude as com- pared with the Stars.	From	To				
1848.	h m			h m				
Nov. 1	8 15	1	α Aquilæ (Altair)	α Cygni	Very bright.	Seerah Island, Aden.	Serjeant W. Mays, H.M. 2nd Cavalry, under Bombay Geographical Society.	See Appendix, No. 3.
4	10 19	3	β Aquarii	In a S.W. direction				
	7 4	2	Zenith	In a N.E. direction				
	8 24	1	About 6° above (Polaris)	Towards N.W. horizon				
	10 35	4	α Persei	Towards N. horizon	Leaving a trail of light for 3".			
5	9 40		α Ceti	S. of Orionis				
7	8 0	2	10° S. of Pleiades	E. horizon				
	8 45	3	N.W. of zenith	Towards N.W. horizon				
	9 0	4	N.W. by W. alt. 45°	N.W. horizon				
	9 16	2	Aldebaran	α Orionis	Comet-like tail.			
	9 35	1	α Cassiopeiæ	W. of Polaris				
	10 11	5	β Orionis	S.E. direction				
	12 4	3	Canopus	S.W.				
8	9 35	4	β Andromedæ	T Andromedæ or Almahc ..				
11	6 54	1	5° above Altair	W. horizon	Followed by a smaller one.			
13	8 4	4	S.E. alt. 25°	S.E. horizon				
	8 54	5	S. alt. 45°	S. horizon				
	9 56	3	β Cassiopeiæ	N. horizon				
16	7 20	1	15° above Polaris	N.W. horizon	Leaving a train of light visible 6".			
	7 23	4	A little above Polaris	N.W. direction				
	7 30	1	About 4° above Polaris	N.W. horizon				
	8 10	4	S. 20° in alt.	S.W. horizon				
	9 40	3	Belt of Orion	E. horizon				
	13 15	3	W. alt. 45°	W. horizon				
18	9 33	1	S. of zenith	Towards W. horizon	[light.			
20	7 20	1	β Ceti	S.W. horizon	Leaving a light-blue train of			
	8 30	1	E. of Pleiades	η Eridanæ	Very bright.			
21	7 40	3	E. of zenith	Towards E. horizon				
	8 20	1	E. of zenith	α Orionis				
	12 15	1	Above Polaris	N.W. horizon				
	15 0	3	E. of Jupiter	E. horizon				
23	7 5	1	α Aquarii	α Gruis				
	8 4	2	Zenith	Altair				
24	8 10	1	Zenith	W.				
26	8 35	2	E. of zenith	E. horizon				
	9 16	2	Belt of Orion	N.E. horizon				
26	9 30	6	S. horizon	S. horizon				

Date.	Description.	Place.	Observer.	Reference.
1848.				
Nov. 9	11 ^h 50 ^m . A fine globe meteor; pale blue; = 5 times $\frac{1}{2}$; from π Pegasi to near Delphinus; perpendicularly down for 30°, when it burst and disappeared.	Highfield House, Nottingham.	E. J. Lowe, Esq.	MS.
13, 14...	From 12 ^h 45 ^m to 3 ^h 15 ^m A.M., on the 14th. 10 meteors. M. Coulvier Gravier, calculating on his system, finds the horary number <i>below</i> the average.	Paris ?	M. Coulvier Gravier.	Comptes Rendus, 1848. ii. 521.
17	1 ^h 15 ^m A.M. A small bolide ...	Ibid.....	Id.	Ibid.
	11 ^h 12 ^m P.M. A meteor with a bright train, moved from about 6° or 8° N. of the Pleiades through the zenith, to about 30° or 35° above the horizon, about N.N.W., when it disappeared; in about 4 seconds a bright rose-coloured aurora had appeared during the evening, and at this time assumed the appearance of beams converging towards the zenith. The course of the meteor was exactly along one of these beams.	Oxford	Mr. G. A. Rowell.	Verbal statement to Prof. Powell.
	<i>During an aurora.</i> A bright blue-globe meteor fell from Capella towards the N. horizon, leaving a "stream of stars."	Mr. Lawson's Observatory, Bath.	E. J. Lowe, Esq.	MS.
	11 ^h 4 ^m . A small falling star from the cupola of the aurora (then situated at 21 Persei), to near the W. horizon; seemed brighter than usual for their size, and very rapid. One from zenith through 80° in 1½ second.	Ibid.....	Id.	Ibid.
21	During an aurora, four small meteors fell into the aurora and disappeared.	Rosehill, Oxford.	Rev. J. Slatter...	Letter to Prof. Powell.
29	19 ^h 0 ^m 50 ^s 35. Grantham mean time of disappearance. [The time taken from transit of β Leonis; my long. I reckon 2 ^m 36 ^s W. from London; but this perhaps 4 or 6 sec. too great.] A meteor much larger and brighter than $\frac{1}{2}$ moved nearly horizontally about 10° while seen; moderate velocity; colour pale rose. Vanished after 1 second, almost perpendicularly below $\frac{1}{2}$, and about four times as far from $\frac{1}{2}$ as $\frac{1}{2}$ from Regulus. No train; but perhaps invisible from light.	Grantham, Lincolnshire.	J. W. Jeans, Esq.	Mr. Lowe's MS.

Date	Description.	Place.	Observer.	Reference.
1848.				
Nov. 30	11 ^h . A fine caudate meteor through γ Gemini.	Highfield Ho., Nottingham.	E. J. Lowe, Esq.	MS.
Dec. 13	11 ^h . A small falling star near Aldebaran.	Ibid.....	Id.	Ibid.
1849.				
Jan. 28	8 ^h 15 ^m P.M. A meteor = twice $2\frac{1}{2}$; blue, with slight train of sparks; moved slowly from Castor to near Regulus.	Bath. Observatory of H. Lawson, Esq.	Mr. and Mrs. Lowe.	Ibid.
Feb. 10	A bright meteor	Hld. Ho., Not.	A. S.H. Lowe, Esq.	Ibid.
24	A meteor	Madras	A Correspondent	Bombay Times.
28	10 ^h 15 ^m P.M. During an aurora, a meteor with train; lost behind clouds at about 15° alt.	Rosehill, Oxford.	Rev. J. Slatter...	MS.
March 6	6 ^h 8 ^m P.M. A brilliant white large meteor fell from a little below and S. of the moon, and after 1 $\frac{1}{2}$ sec. exploded with a greenish and red flash, at about 12° alt. in S.E.	Mill-yard, Whitechapel, London.	W. H. Black, Esq.	Letter to Prof. Powell. Appendix, No. 8.
19	6 ^h 30 ^m P.M. A brilliant green meteor; direction N.E.; exploded and separated into red particles or sparks. [Some discrepancies in the observations at different places render it probable that two distinct meteors were seen.]	Bombay, Poona, Aurangabad, Sholapoor, Surat, and other places.	Numerous Observers.	Bombay Times. Appendix, No. 9.
23	A small meteor	Bombay	A Correspondent	Ib. Ib. No. 9.
26	A bright meteor = Venus; nucleus green; tail red; direction N.W.; burst into fragments.	Cochin.....	Id.	Ib. Ib. No. 10.
April 4	A meteor.....	Delhi	Id.	[July 11, 1849. Bombay Times,
10	One large meteor and 2 small...	Ahmednuggur	Id.	Ibid.
13	A fine meteor, from near Spica Virginis to S. horizon.	Highfield Ho., Nottingham.	S. Watson, Esq.	Mr. Lowe's MS.
	9 ^h P.M. Brilliant, with blue train from W. to S.E.; no explosion.	Bombay and Hingolee.	Several Observers.	Bombay Times. Appendix, No. 11.
19? or 26?	Brilliant; burst into sparks ...	Bombay	A Correspondent	Ib. Ib. No. 12.
30	Bright; no explosion.....	Poona	Id.	Ib. Ib. No. 12.
20	11 ^h P.M. A small falling star, downwards from γ Libræ.	Highfield Ho., Nottingham.	E. J. Lowe, Esq.	MS.
	11 ^h 15 ^m . Small; perpendicularly from Aldebaran; rapid; left streamers.	Ibid.....	Id.	Ibid.
	11 ^h 19 ^m . Small; vertically upwards from near μ Lyræ.	Ibid.....	Id.	Ibid.
	11 ^h 20 ^m . Small; downwards; inclined about 45° from δ Virginis to near ϵ Crateris.	Ibid.....	Id.	Ibid.
	11 ^h 24 ^m . One; from λ Urs. Maj. through 37 Leonis Minor.	Ibid.....	Id.	Ibid.
	11 ^h 25 ^m . One from Dubbe to χ Urs. Maj.	Ibid.....	Id.	Ibid.
	11 ^h 27 ^m . One; perpendicularly down from Coma Berenice, passingslightly to E. of Deneb.	Ibid.....	Id.	Ibid.

Date.	Description.	Place.	Observer.	Reference.
1849.				
April 26	11 ^h 59 ^m . Small; horizontally towards Polaris, from δ Draconis to τ Draconis; blue; rapid; left sparks.	Highfield Ho., Nottingham.	E. J. Lowe, Esq.	MS.
	12 ^h . One, from η Urs. Maj. through α Draconis to γ Urs. Min.; yellow; quick.	Ibid.....	Id.	Ibid.
	12 ^h 7 ^m . Small; horizontally from δ Draconis to τ Draconis; blue; rapid; left sparks. [Same track as that at 11 ^h 59 ^m .]	Ibid.....	Id.	Ibid.
	12 ^h 15 ^m . One; horizontally from Atair through δ Aquilæ; rapid; yellow; left sparks.	Ibid.....	Id.	Ibid.
28	11 ^h 7 ^m . Small; just above β & γ Draconis.	Ibid.....	Id.	Ibid.
30	11 ^h 8 ^m . One, crossed ξ Draconis.	Ibid.....	Id.	Ibid.
	8 ^h P.M. Meteor of elongated shape; brightness = 1st mag.; slow, with train; descended to N.W. horizon; inclined at 45°.	Liège	M. Koninck.....	L'Institut, No. 808, p. 206.
	Bright meteor; no explosion....	Poona	A Correspondent	[pendix, No. 12. Bombay Times, Ap- Ibid.
May 2	A meteor.....	Bombay	Id.	Ibid.
6	A meteor.....	Kurrachee ...	Id.	Ib. July 11, 1849.
8	9 ^h 18 ^m P.M. A globe meteor from ϵ Herculis to β Lyræ. [These positions of its path were estimated from α Lyræ, since the full moon prevented smaller stars being seen.] Motion slow; colour red; size and brilliancy about = α Lyræ, the moon shining on both; path behind light cirri; no streamers.	Highfield Ho., Nottingham.	Rev. K. Swann...	Mr. Lowe's MS.
11	11 ^h 16 ^m . Falling star near Spica Virginis.	Ibid.....	E. J. Lowe, Esq.	MS.
	11 ^h 33 ^m . Bright falling star; yellow; small train; from α Draconis through χ to λ Draconis.	Ibid.....	Id.	Ibid.
13	10 ^h 3 ^m . Do. small; slight train; yellow; rapid; from σ to between Capella and β Aurigæ.	Ibid.....	Id.	Ibid.
	11 ^h 9 ^m . Do. small; rapid; from α Draconis to Coma Berenicens.	Ibid.....	Id.	Ibid.
22	11 ^h 11 ^m . Do. small; rapid; yellow; from Muriack to between Arcturus and η Bootis.	Ibid.....	Id.	Ibid.
June 13	12 ^h . Do. = Sirius; from Algenib to α Arietis.	Ibid.....	A. S. H. Lowe, Esq.	Ibid.
16	11 ^h 28 ^m . Do. = 1st mag.; rapid; yellow; from 5° below Polaris, downwards to 20° slightly W. of 42 Camelopardalis.	Ibid.....	E. J. Lowe, Esq.	Ibid.
	11 ^h 32 ^m . Do. = 3rd mag.; from γ Urs. Min. to ϵ Urs. Maj.; rapid; yellow.	Ibid.....	Id.	Ibid.

Date.	Description.	Place.	Observer.	Reference.
1849.				
June 17	11 ^h 9 ^m 30 ^s . Small falling star; yellow; slow; from 41 Com. Beren. to 1° W. of ϵ Virginis.	Highfield Ho., Nottingham.	E. J. Lowe, Esq.	MS.
	11 ^h 10 ^m . Do. from Arcturus, perpendicularly down to τ Virginis.	Ibid.....	Id.	Ibid.
	9 ^h 12 ^m P.M. A meteor; white tinged, slightly orange; at first=5th mag.; <i>increasing in brightness</i> till brighter than ϕ ; moved E. from near η Aquilæ for about 15°, and disappeared near ϵ Delphini; just before disappearance a fragment detached, and then others smaller, which all followed in same track..	Observatory, Cambridge, U.S.	Prof. Bond	American Journals. Morning Post, July 11, 1849.
25	10 ^h P.M. Very brilliant from S. to W.; exploded at about 60° alt., leaving luminous red fragments; about 5 minutes after sound heard like ordnance.	Kurrachee ...	A Correspondent	Bombay Times, July 11, 1849.
23	11 ^h 25 ^m . = 1st mag.; yellow; rapid from δ Arcturi [Bootis] passing 1° N. of Arcturus, and fading away between τ and ν [Bootis].	Highfield Ho., Nottingham.	E. J. Lowe, Esq.	MS.
	11 ^h 26 ^m 30 ^s . Small; moved 1° horizontally; 3° above ϵ Virginis.	Ibid.....	Id.	Ibid.
27	11 ^h 33 ^m . = γ ; small train; yellow; slowly from α Ophiuchi towards W., curving towards S., through γ Herculis and γ Serpentis.	Ibid.....	Id.	Ibid.
30	11 ^h 43 ^m . = γ ; rapidly from Kochab, between Alioth and Weizar, and faded away near C.H. 120 Canis Venatici; path inclined about 50° to horizon; left train of bright pale red sparks for $\frac{1}{2}$ sec.	Ibid.....	Id.	Ibid.
July 4	11 ^h 59 ^m . = 1st mag.; light red; no train; slowly from α^2 Cygni, down through γ Cygni to γ Equulei.	Ibid.....	Id.	Ibid.
8	11 ^h 26 ^m P.M. Small; yellow; slow; from α to χ Draconis.	Ibid.....	Id.	Ibid.
10	11 ^h 3 ^m P.M. Globe meteor=5 times γ ; nearly pale blue; conical; slow, without sparks, from about η Pegasi; through α Andromedæ to about ϕ Piscium.	Ibid.....	A. S. H. Lowe, Esq.	Ibid.
16	11 ^h 28 ^m . = twice 1st mag.; pale red, with long train; rapid from ω^3 Cygni to α^1 Cygni.	Ibid.....	E. J. Lowe, Esq.	Ibid.
	11 ^h 40 ^m . = 2nd mag.; blue; rapid; through β and α Equulei.	Ibid.....	Id.	Ibid.

Date.	Description.	Place.	Observer.	Reference.
1849. July 20	12 ^h 4 ^m . = 1st mag.; yellow, with stream; slow from No. 3 Lacertæ to γ Cassiopeia.	Highfield Ho., Nottingham.	E. J. Lowe, Esq.	MS.
	12 ^h 12 ^m . Small, with streamers; rapid from ϵ Cassiopeia; inclined at 45° downwards towards N.	Ibid.....	Id.	Ibid.
	12 ^h 16 ^m . Small, with streamers; pale red; rapid from H 6 and H 8 Camelopardalis to α Persei.	Ibid.....	Id.	Ibid.
	12 ^h 22 ^m . Small; red, with tail; slowly downwards at about 45°; from 2° S of h_2 , and about the same alt. through 3°.	Ibid.....	Id.	Ibid.
	12 ^h 32 ^m . Small; no tail; yellow; rapid; from β Pegasi to χ Andromedæ.	Ibid.....	Id.	Ibid.
21	10 ^h 54 ^m . Small; rapid; through 30' to N. from α Cephei.	Ibid.....	Id.	Ibid.
	11 ^h 5 ^m . = 2nd mag.; yellow; rapidly from ω^1 , ω^2 and ω^3 Cephei to α Cephei.	Ibid.....	Id.	Ibid.
	11 ^h 20 ^m . Do. small; light red; rapidly from Delphinus, nearly perpendicularly down to 71 Antioi.	Ibid.....	Id.	Ibid.
	11 ^h 25 ^m . = 1st mag.; red; train of sparks; rapid; nearly horizontally towards S.W. from Dubbe, to 1° below Alioth.	Ibid.....	Id.	Ibid.
	11 ^h 42 ^m . = 1st mag.; red; splendid stream of light, 5° in length; slow; from λ Andromedæ to μ Cassiopeia.	Ibid.....	Id.	Ibid.
	11 ^h 45 ^m . Sm.; red, with stream; rapidly from μ Cassiopeia.	Ibid.....	Id.	Ibid.
	11 ^h 47 ^m . Small; yellow, with tail; rapid; inclined at 45° towards S. from δ Pegasi to μ Aquarii.	Ibid.....	Id.	Ibid.
	11 ^h 49 ^m . = 1st mag.; dark green; rapidly inclined at 45° towards S. from ζ Pegasi to γ Aquarii.	Ibid.....	Id.	Ibid.
22	From 9 ^h 30 ^m to 10 P.M. 5 meteors.	Jersey	Rev. J. Slatter...	[Powell. Letter to Prof.
23	10 ^h 37 ^m P.M. A meteor about = 1st mag.; white, with train; appeared from behind buildings; passed below Arcturus and η Bootis, parallel to the line joining them, and at a distance nearly = that of those stars from each other; disappeared without explosion, at a distance below and beyond η Bootis, about double its distance from α , which gives (by the U.K.S. star map) for the point of disappearance about RA. 200°; Decl. 9° N.	Oxford.....	Prof. Powell.....	

Date.	Description,	Place.	Observer.	Reference.
1849.				
July 23	10 ^h 40 ^m 30 ^s . Small; yellow; no tail; rapidly from α Cephei to Polaris.	Highfield Ho. Nottingham.	E. J. Lowe, Esq.	MS.
	10 ^h 42 ^m 30 ^s . Small; red; slight tail; rapidly from ξ Cygni to α Cephei.	Ibid.....	Id.	Ibid.
	10 ^h 43 ^m . Small; from π Cassiopeia to η Persei.	Ibid.....	Id.	Ibid.
	10 ^h 45 ^m . Small; red; tail; rapidly from γ Sagittæ to γ Aquilæ.	Ibid.....	Id.	Ibid.
	10 ^h 46 ^m . Small; yellow; no tail; rapidly from τ Cassiopeia to χ Andromedæ.	Ibid.....	Id.	Ibid.
	10 ^h 49 ^m . Small; yellow; slight tail; rapidly from 40 ^o Draconis to H 4 Draconis.	Ibid.....	Id.	Ibid.
	11 ^h 0 ^m . Small; yellow; no tail; rapidly from ζ Cygni to π Pegasi.	Ibid.....	Id.	Ibid.
	11 ^h 7 ^m . Small; red; tail; rapidly from β Cephei to γ Cephei.	Ibid.....	Id.	Ibid.
	11 ^h 10 ^m 30 ^s . Fine; = 1st mag.; light green; no tail; rapidly from No. 7 Camelopardalis to β Aurigæ.	Ibid.....	Id.	Ibid.
	11 ^h 20 ^m . Small, with stream; yellow; rapidly from λ Androm. to 1 ^o below π Cassiopeia.	Ibid.....	Id.	Ibid.
	11 ^h 21 ^m . Small; red; tail; rapidly from α to η Pegasi.	Ibid.....	Id.	Ibid.
	11 ^h 23 ^m 30 ^s . Small; yellow; no tail; rapidly from π Pegasi to ψ Andromedæ.	Ibid.....	Id.	Ibid.
	11 ^h 29 ^m . = 2nd mag.; red, with streamers; slow; from α Cephei to η Persei.	Ibid.....	Id.	Ibid.
	11 ^h 31 ^m . = 1st mag.; dark red; streamers; quickly from 7 Lacertæ to χ Andromedæ.	Ibid.....	Id.	Ibid.
	11 ^h 32 ^m . = 2nd mag.; yellow; no tail; rapidly from ϵ Cephei to γ Cassiopeia.	Ibid.....	Id.	Ibid.
	11 ^h 33 ^m . Small; yellow; slight tail; rapidly from α Cephei to H 33 Cygni.	Ibid.....	Id.	Ibid.
	11 ^h 34 ^m . Small; train; yellow; rapid from η Cephei to δ Cephei.	Ibid.....	Id.	Ibid.
	11 ^h 40 ^m . = 3rd mag.; red; stream; rapidly from χ Androm. to 56 Pegasi.	Ibid.....	Id.	Ibid.
27	10 ^h 40 ^m . Small; rapid; in the line from η Urs. Maj. to η Bootis; disappeared a little above the latter.	Oxford.....	Prof. Powell.	
24	11 ^h 22 ^m 30 ^s . Small, with tail; blue; rapidly from 43 Camelopardalis to τ Urs. Majoris.	Highfield Ho. Nottingham.	E. J. Lowe, Esq.	MS.

Date.	Description.	Place.	Observer.	Reference.
1849.				
July 24	11 ^h 24 ^m 30 ^s . Small; blue, with streamers; rapidly; from H 35 Cassiopeiae to C.H. 155 Camelopardalis.	Highfield Ho., Nottingham.	E. J. Lowe, Esq.	MS.
	11 ^h 29 ^m . Yellow; slight streamers; rapid; from γ Persei to H. 14 Camelopardalis.	Ibid.....	Id.	Ibid.
26	11 ^h 37 ^m . = 3rd mag with a continuous streak of blue; rapidly from ξ Cephei to 3° beyond H 43 Cephei.	Ibid.....	Id.	Ibid.
	11 ^h 42 ^m . Small; yellow; no tail; swiftly from α Aquarii to 71 Antinoi.	Ibid.....	Id.	Ibid.
	11 ^h 43 ^m . Small; continuous red train; rapidly from 29 Vulpeculae to near ρ Delphini.	Ibid.....	Id.	Ibid.
	11 ^h 46 ^m . Small; no tail; swiftly from λ to α Pegasi.	Ibid.....	Id.	Ibid.
	11 ^h 59 ^m . Small; blue, with continuous stream; rapidly; perpendicularly down from 86 Pegasi, through 35 and 36 Piscis.	Ibid.....	Id.	Ibid.
	12 ^h 1 ^m . Small; rapidly from ν Cygni to Delphinus.	Ibid.....	Id.	Ibid.
	12 ^h 7 ^m . = 1st mag; having continuous bright red streak; afterwards broken into streamers; from 17 Vulpeculae to Vega.	Ibid.....	Id.	Ibid.
	12 ^h 14 ^m . Very small; yellow; with continuous ray, nearly perpendicularly down; rapidly from γ Pegasi to δ Piscis.	Ibid.....	Id.	Ibid.
	12 ^h 19 ^m . Small, with continuous blue ray; rapid; nearly horizontal; from θ Persei to a little above α Persei.	Ibid.....	Id.	Ibid.
27	11 ^h 5 ^m . Small; rapid; from 109 Herculis through α Ophiuchi.	Ibid.....	Id.	Ibid.
	11 ^h 10 ^m . Small; yellow; no tail; rapid; from ξ Andromedae to β Arietis.	Ibid.....	Id.	Ibid.
	11 ^h 11 ^m . Small; no tail; light red; rapid; from γ Cephei to δ Urs. Min.	Ibid.....	Id.	Ibid.
	11 ^h 15 ^m . = 3rd mag.; red; tail; rapid; from γ Cephei to δ Urs. Min.	Ibid.....	Id.	Ibid.
	11 ^h 22 ^m . Small; tail; yellow; rapidly from H 3 Camelopardalis to a little N. of Capella.	Ibid.....	Id.	Ibid.
	11 ^h 25 ^m . = 4th mag.; yellow, with streamers; from 57 Cygni, between γ and ξ Draconis, and then between β and double star ν 1 and ν 2 Draconis.	Ibid.....	Id.	Ibid.

Date.	Description.	Place.	Observer.	Reference.
1849.				
Aug. 3	10 ^h = 2; pale; from χ Cassiopeia to β Persei.	Highfield Ho., Nottingham.	A. S. H. Lowe, Esq.	MS.
6	10 ^h 30 ^m . = 1st mag.; long train; rapid; from α Aquilæ to α Ophiuchi.	Ibid.....	E. J. Lowe, Esq.	Ibid.
	8 ^h 35 ^m P.M. (twilight). A bright meteor=5 or 6 times Vega; estimated to fall from near δ Cygni to 5°W. of β Aquarii; there extinguished, leaving sparks, which vanished a little below the same point.	Rosehill, near Oxford.	Rev. J. Slatter...	MS. Letter. Appendix, No. 13.
	10 ^h 45 ^m . = 2nd mag., from near Polaris, in the direction of δ Urs. Maj.; lost behind buildings.	Oxford.....	Prof. Powell.	
7	9 ^h 30 ^m . A globe meteor=2; purple; no train or sparks; slow; from ζ Cygni through f and g Pegasi; several small.	Highfield Ho., Nottingham.	E. J. Lowe, Esq.	MS.
8	9 ^h 20 ^m . = 1st mag.; blue; rapid, with sparks; from β Persei to γ Trianguli.	Ibid.....	Mrs. Lowe	Ibid.
	9 ^h 52 ^m . Small; yellow; rapid; from close above χ Lyrae to ζ Herculis, leaving a line of light.	Ibid.....	E. J. Lowe, Esq.	Ibid.
	9 ^h 57 ^m . Small; yellow, with sparks; swift; from ϵ Cassiopeia to H 4 Camelopardalis.	Ibid.....	Id.	Ibid.
	10 ^h 16 ^m . A splendid meteor, > 2 \times 1st mag.; orange-red, with a train of sparks and conical head; slow; horizontally; from ζ Bootis, passing 1° below Arcturus; disappeared for about 1 sec., then continued in same track for about 1½°.	Ibid.....	Id.	Appendix, No. 14.
	10 ^h 19 ^m . = 2nd mag.; orange-red; no tail; rapid; from 30' above α Urs. Maj. through γ Urs. Maj.	Ibid.....	Id.	Ibid.
	10 ^h 27 ^m . Small; rapid; from 56 Cygni to 11 Vulpeculæ.	Ibid.....	Id.	Ibid.
	9 ^h 20 ^m P.M. Small; no train; rather slow; from ζ Lyrae to 109 Herculis.	Castle Donington, Leicestershire.	W. H. Leeson, Esq.	Communicated by Mr. Lowe.
	9 ^h 21 ^m . Very bright, and much larger than α Lyrae; from near the small star p Tauri Ponictowski to 17 Lyrae; train remained visible about 2½ secs.	Lat. 52° 51' 23'' 75 N., Long. 1° 18' 42'' W.	Id.	Ibid.
	9 ^h 25 ^m . Small; slow; no train; from 106 Herculis to α Lyrae.	Ibid.....	Id.	Ibid.
9	12 ^h 0 ^m . Yellow; = 2nd mag.; very rapid; no tail; from H 15 Urs. Maj. to ψ Urs. Maj. From 9 ^h to 11 ^h . Six small meteors; direction S. and S.W.	Highfield Ho., Nottingham. Gosport	E. J. Lowe, Esq. H. Burney, Esq.	MS. Communicated by Mr. Lowe.

Date.	Description,	Place.	Observer.	Reference.
1849.				
Aug. 10	From 9 ^h to 10 ^h . Five small meteors; two with trains; towards S. and W.	Gosport	H. Burney, Esq..	Communicated by Mr. Lowe.
	Till 11 ^h 30 ^m . Entirely cloudy; clear later in the night; some meteors reported.	Oxford.....		
	Cloudy till 9 ^h 40 ^m , thence to 9 ^h 55 ^m ; about 10 meteors.	Highfield Ho., Nottingham.	E.J. Lowe, Esq., Mrs. Lowe, A. Lowe, Esq., A. S. H. Lowe, Esq., F. E. Swann, Esq., and an Assistant.	MS.
	9 ^h 56 ^m . = 2nd mag.; pale-red, with stream of light; brightest at centre, and fading away towards its two extremities; rapid; from ξ Cygni to χ Draconis.			
	9 ^h 58 ^m . Similar, but fainter; in same track.	Ibid.....	Id.	Ibid.
	9 ^h 55 ^m . = 1st mag.; well-defined disc; no tail; rapid; from α Andromedæ to α Pegasi.	Ibid.....	Id.	Ibid.
	10 ^h 0 ^m . Similar; fainter; in same track.	Ibid.....	Id.	Ibid.
	10 ^h 2 ^m . Yellow, with tail; very rapid; from ϵ Cephei to α Cygni.	Ibid.....	Id.	Ibid.
	10 ^h 3 ^m . = φ at brightest; yellow, with streamers; brightest in the middle; slow; from χ Antinoi through κ Antinoi to about χ Sagittarii.	Ibid....	Id.	Ibid.
	10 ^h 4 ^m . Nearly similar; in same track.	Ibid.....	Id.	Ibid.
	10 ^h 5 ^m 30 ^s . = φ at present; globe meteor; very rapid; from ϵ Aquilæ to λ Antinoi. After disappearance a pale-red ray of light along the last 7° of the track; lasted 31 seconds.	Ibid.....	Id.	Ibid.
	10 ^h 6 ^m . Small; in same track.	Ibid.....	Id.	Ibid.
	10 ^h 7 ^m . Small; yellow; rapid; from Delphinus to 69 Antinoi.	Ibid.....	Id.	Ibid.
	10 ^h 7 ^m . Small; yellow; rapid; from g Pegasi to a little south of α Aquarii.	Ibid.....	Id.	Ibid.
	10 ^h 7 ^m 30 ^s . Small; in same track.	Ibid.....	Id.	Ibid.
	10 ^h 8 ^m . = η ; pale red; rapid; from χ Delphini to θ Antinoi. Continued ray lasted 2 secs. after disappearance.	Ibid.....	Id.	Ibid.
	10 ^h 8 ^m 30 ^s . Small; in same track.	Ibid.....	Id.	Ibid.
	10 ^h 9 ^m . Small; yellow; rapid; from α Cygni through τ Cygni.	Ibid.....	Id.	Ibid.
	10 ^h 9 ^m 15 ^s . = η ; pale blue; defined disc; rapid; from between α and γ Cygni to 1° S. of ϵ Delphini; stream of light 1° after disappearance.	Ibid.....	Id.	Ibid.

Date.	Description.	Place.	Observer.	Reference.
1849.				
Aug. 10	10 ^h 11 ^m . Globe meteor= ♀ at present; deep blue; slow; from 1° below Polaris to α Draconis; disappeared suddenly, not breaking up into fragments; blue streak 10° in length; lasted 4 secs. after disappearance.	Highfield Ho., Nottingham.	E. J. Lowe, Esq. &c. &c.	MS.
	10 ^h 11 ^m 15 ^s . = 2; pale red; parallel to last; about ½° below.	Ibid.....	Id.	Ibid.
	10 ^h 13 ^m . = 1st mag.; yellow; rapid; from midway between α Andromedæ and α Pegasi to γ Piscis. Streak along whole path; lasted 1 sec. after disappearance.	Ibid.....	Id.	Ibid.
	10 ^h 14 ^m . Small; in same track.	Ibid.....	Id.	Ibid.
	10 ^h 16 ^m . = 1st mag.; blue; rapid; from the Galaxy near Cygnus, ½° below Vega to χ Lyræ; ray visible 2 secs. after disappearance.	Ibid.....	Id.	Ibid.
	10 ^h 17 ^m . Small; with tail; rapid; from near β to about ξ Pegasi.	Ibid.....	Id.	Ibid.
	10 ^h 17 ^m 15 ^s . Similar; in same track.	Ibid.....	Id.	Ibid.
	10 ^h 17 ^m 30 ^s . Do. do.....	Ibid.....	Id.	Ibid.
	10 ^h 19 ^m . = 1st mag.; red; rapid; from 2° N. of Arcturus, through 42 Coma Berenicens.	Ibid.....	Id.	Ibid.
	10 ^h 20 ^m . Small; rapid; from β to γ Pegasi.	Ibid.....	Id.	Ibid.
	10 ^h 22 ^m 30 ^s . = 2nd mag.; yellow; tail; rapid; from θ Antinoi downwards, inclining S.	Ibid.....	Id.	Ibid.
	10 ^h 23 ^m 30 ^s . = 2; tail; slow; from N. of Lyra to τ Draconis.	Ibid.....	Id.	Ibid.
	10 ^h 24 ^m . Small; yellow; rapid; from Polaris towards Vega.	Ibid.....	Id.	Ibid.
	10 ^h 24 ^m 30 ^s . Small; rapid; from Corona Borealis to χ Serpentis.	Ibid.....	Id.	Ibid.
	10 ^h 25 ^m . Small; tail; rapid; from γ through σ Aquarii.	Ibid.....	Id.	Ibid.
	10 ^h 25 ^m 30 ^s . = 1st mag.; yellow; rapid; from h Cassiopeie to Polaris; ray visible 1 sec.	Ibid.....	Id.	Ibid.
	10 ^h 26 ^m 30 ^s . Small; rapid; from λ to θ Pegasi.	Ibid.....	Id.	Ibid.
	10 ^h 28 ^m . Small; rapid; from No. 3 Aquarii through α Capricorni.	Ibid.....	Id.	Ibid.
	10 ^h 30 ^m . Two; very small; followed each other rapidly from Cygnus to Lyra.	Ibid.....	Id.	Ibid.
	10 ^h 31 ^m . = 1st mag.; yellow; rapid, and with tail; from χ Andromedæ to λ Pegasi.	Ibid.....	Id.	Ibid.

Date.	Description.	Place.	Observer.	Reference.
1849. Aug. 10	10 ^h 34 ^m . = 1st mag.; rapid; from α Cygni to β Lyræ; continuous white ray, 1 sec. after disappearance.	Highfield Ho., Nottingham.	E. J. Lowe, Esq. &c. &c.	MS.
	10 ^h 34 ^m 30 ^s . Smaller, but similar; from λ Cygni to σ Herculis.	Ibid.....	Id.	Ibid.
	10 ^h 35 ^m . Small; rapid, down- wards from γ Cassiopeiæ.	Ibid.....	Id.	Ibid.
	10 ^h 35 ^m 30 ^s . = μ ; blue; de- fined disc; brilliant; from 27 Urs. Maj. to 0° 34' above α Urs. Maj.; blue continuous streak.	Ibid.....	Id.	Ibid.
	10 ^h 36 ^m . Small; yellow; rapid; from γ Cassiopeiæ to η Persei.	Ibid.....	Id.	Ibid.
	10 ^h 36 ^m 30 ^s . Small; rapid; from H 32 Camelopardalis to 7 Draconis.	Ibid.....	Id.	Ibid.
	10 ^h 38 ^m . Small; rapid; from 81 Urs. Maj. to λ Bootis.	Ibid.....	Id.	Ibid.
	10 ^h 38 ^m 30 ^s . Similar; in same track.	Ibid.....	Id.	Ibid.
	10 ^h 40 ^m . Small; rapid; from H 43 Cephei to H 32 Camelo- pardalis; ray of light.	Ibid.....	Id.	Ibid.
	10 ^h 44 ^m . Small; rapid; from χ Corona Borealis to δ Bootis.	Ibid.....	Id.	Ibid.
	10 ^h 44 ^m 30 ^s . = ζ at brightest; pale yellow; rapid; from ν Andromedæ to between α and β Trianguli; ray visible 1 sec. after disappearance.	Ibid.....	Id.	Ibid.
	10 ^h 45 ^m . = 1st mag.; yellow; rapid; from 3 Urs. Min. to C.H. 122 Urs. Maj.; left train.	Ibid.....	Id.	Ibid.
	10 ^h 45 ^m 30 ^s . = ζ at brightest; yellow; rapid; from β Andro- medæ through ϕ Piscis.	Ibid.....	Id.	Ibid.
	10 ^h 46 ^m . Small; rapid; from just above τ downwards, in- clining to S.	Ibid.....	Id.	Ibid.
	10 ^h 47 ^m . Small; rapid; from nebula in Androm., just above ν to 17 Andromedæ.	Ibid.....	Id.	Ibid.
	10 ^h 49 ^m . Small; rapid; from γ Urs. Min. to η Draconis.	Ibid.....	Id.	Ibid.
	10 ^h 50 ^m 30 ^s . Small; rapid; from χ Cassiop. to h Cassiop.	Ibid.....	Id.	Ibid.
	10 ^h 52 ^m . = ζ ; red; defined disc; slow; from ϵ Persei downwards; no train; after this cloudy.	Ibid.....	Id.	Ibid.
	Between 9 ^h 30 ^m and 9 ^h 33 ^m P.M. A rather large meteor, from between Cygnus and Cassio- peia, to between Cygnus and Pegasus; left reddish train of sparks; brightest at mid. part.	[London. Bethnal Green,	W. R. Birt, Esq.	Appendix, No. 18.

Date.	Description.	Place.	Observer.	Reference.
1849. Aug. 10	Small meteor, from 1° or 2° E. of Polaris, downwards.	Bethnal Green, London.	W. R. Birt, Esq.	Appendix, No. 18.
	Small; rapid; very obliquely across the line joining α and β Pegasi.	Ibid.....	Id.	Ibid.
	Small; near head of Capricorn; direction S.W.	Ibid.....	Id.	Ibid.
	$9^h 55^m$ (?) Rather large; from N. of Cassiopeia to a little N. of Cygnus; train of reddish sparks; brightest at mid. part.	Ibid.....	Id.	Ibid.
	$10^h 5^m$ (?) Very large and bright; from below Ursa Major to S. of Corona Borealis; reddish train.	Ibid.....	Id.	Ibid.
	—Small, but bright; through Polaris.	Ibid.....	Id.	Ibid.
	—Globular meteor = 4 ; reddish; slow; through γ Pegasi; increased in brightness.	Ibid.....	Id.	Ibid.
	—Another, exactly similar, after about one minute, in prolongation of same path.	Ibid.....	Id.	Ibid.
	From $9^h 19^m$ to $10^h 33^m$, fifty-five shooting stars were observed in such rapid succession that it was found impossible to note the exact positions of the whole of them. Occasionally they much resembled a shower of rockets, shooting in all possible directions. The following are the chief:—	Castle Donington, Leicestershire, lat. $52^{\circ} 51' 23''$ N.; long. $1^{\circ} 18' 42''$ W.	W. H. Leeson, Esq.	Communicated by Mr. Lowe.
	$9^h 55^m$. = Altair; very brilliant; rather slow; from γ Aquilæ to α Delphini; train visible 2 seconds.	Ibid.....	Id.	Ibid.
	$10^h 1^m$. Somewhat quicker; no train; from σ Herculis to a little above η Lyrae.	Ibid.....	Id.	Ibid.
	$10^h 10^m$. Two together; one much brighter than the other; moved uniformly down the Milky Way from ϵ Cygni to α and γ Sagittæ; the brighter appeared to terminate its course in a zigzag form, leaving a small train; the other none.	Ibid.....	Id.	Ibid.
	$10^h 11^m$. Very brilliant; $>1^{\text{st}}$ mag.; from Polaris to χ Serpentis; train visible 3 secs.; rapid; bluish white; cast a visible shadow.	Ibid.....	Id.	Ibid.
	$10^h 12^m$. Brighter than 1^{st} mag.; rather slow; from Deneb to α Lyrae.	Ibid.....	Id.	Ibid.
	$10^h 15^m$. Bright; straw-colour; rather slow; from Deneb to ζ Lyrae.	Ibid.....	Id.	Ibid.

Date.	Description.	Place.	Observer.	Reference.
1849.				
Aug. 10	10 ^h 21 ^m . Brilliant; from Deneb to Aldebaran.	Castle Donington, &c. &c.	W. H. Leeson, Esq.	Communicated by Mr. Lowe.
	10 ^h 23 ^m . Two together; one from near γ Cygni to π^2 Cygni, crossed the path of the other at right angles, just below Deneb; both = 2nd mag.; slow.	Ibid.....	Id.	Ibid.
	10 ^h 24 ^m . <i>A stream of meteors</i> in parallel lines; from γ Cephei to β Cassiopeiae, about 30' apart; slow.	Ibid.....	Id.	Ibid.
	10 ^h 26 ^m . Very light; seen through thin clouds; down the Milky Way from Deneb to midway between β Cygni and δ Vulpeculae; train visible 3 secs.	Ibid.....	Id.	Ibid.
	10 ^h 30 ^m . = 3rd mag.; no train; from β Cassiopeiae to Schedari.	Ibid.....	Id.	Ibid.
	10 ^h 31 ^m . Very brilliant; from ξ Cygni to ζ Lyrae; train visible through thin clouds.	Ibid.....	Id.	Ibid.
	10 ^h 33 ^m . Large, but obscured by clouds, from $\frac{1}{2}^\circ$ beyond Polaris to γ Urs. Min. After this cloudy.	Ibid.....	Id.	Ibid.
11	Overcast	Ibid.....	Id.	Ibid.
	10 ^h . = 1st mag.; yellow; rapid; from τ Pegasi to about 1° above, and thence to ξ Pegasi; left streak.	Highfield Ho., Nottingham.	E. J. Lowe, Esq.	MS.
	11 ^h 1 ^m . = 2nd mag.; yellow; tail; rapid; from γ Sagittae through γ Aquilae to between σ and μ Aquilae. Much vivid lightning in S.	Ibid.....	Id.	Ibid.
	Cloudy; coruscations over N. and W. horizon.	Oxford.....	Prof. Powell.	
12	10 ^h 9 ^m . Small; from Via Lactea close to Delphinus, upwards to α Cygni.	Highfield Ho., Nottingham.	E. J. Lowe, Esq.	MS.
	10 ^h 10 ^m . = 2nd mag.; yellow; rapid; tail; from ζ Pegasi to γ Aquarii.	Ibid.....	Id.	Ibid.
	10 ^h 13 ^m . Small; from Corona Borealis to λ Serpentis.	Ibid.....	Id.	Ibid.
	10 ^h 14 ^m . = 1st mag.; yellow; tail; rapid; from α Draconis to λ Corona Borealis.	Ibid.....	Id.	Ibid.
	10 ^h 16 ^m . Small; from g Draconis to τ Corona Borealis.	Ibid.....	Id.	Ibid.
	10 ^h 20 ^m . Small; rapid; from γ Urs. Maj. through λ Bootis.	Ibid.....	Id.	Ibid.
	10 ^h 21 ^m . = 1st mag.; yellow; no tail; rapid; from Vega through 110 and 111 Herculis.	Ibid.....	Id.	Ibid.
	10 ^h 22 ^m . Small; rapid; no tail; from χ Urs. Majoris to Arcturus.	Ibid.....	Id.	Ibid.

Date.	Description.	Place.	Observer.	Reference.
1849.				
Aug. 12	10 ^h 23 ^m . Small; rapid; from 11 15 Urs. Maj. through 66 and 3 Urs. Maj.	Highfield Ho., Nottingham.	E. J. Lowe, Esq.	MS.
	10 ^h 23 ^m . = η ; blue; rapid; from ζ Cassiopeia to H 18 Camelopardalis; blue streak; visible for 5 secs.	Ibid.....	A. H. S. Lowe, Esq.	Ibid.
	10 ^h 25 ^m . = 1st mag.; yellow; rapid; from β to ξ Pegasi.	Ibid.....	Id.	Ibid.
	10 ^h 36 ^m . Small; rapid; from 28 and C.H. 153 Urs. Maj. to ν Urs. Maj.	Ibid.....	E. J. Lowe, Esq.	Ibid.
	10 ^h 38 ^m . Small; rapid; from ζ to γ Urs. Maj.	Ibid.....	Id.	Ibid.
	10 ^h 38 ^m . Small; from δ Urs. Maj. through 11 Canium Venet.	Ibid.....	Id.	Ibid.
	{ These two crossed each other's paths about 1° above No. 1 Can. Venet. }			
	10 ^h 42 ^m . Small; rapid; from τ Hercules to β Bootis.	Ibid.....	Id.	Ibid.
	10 ^h 46 ^m . Small; yellow; tail; rapid; from C.H. 155 Camelopardalis to Capella.	Ibid.....	Id.	Ibid.
	10 ^h 46 ^m 30 ^s . Small; rapid; from η Hercules to β Bootis.	Ibid.....	Id.	Ibid.
	10 ^h 47 ^m . Small; rapid; from Corona Borealis to γ Serpentis.	Ibid.....	Id. ...	Ibid.
	10 ^h 56 ^m . = 4; red; rapid; from λ to 31 Pegasi; left ray visible 3 secs.	Ibid.....	Id.	Ibid.
	11 ^h 1 ^m . Small; rapid; from γ Urs. Min. horizontally to η Draconis.	Ibid.....	Id.	Ibid.
	11 ^h 5 ^m . Small, with streamers; rapid; from 31 to 3° beyond 36 Pegasi.	Ibid.....	Id.	Ibid.
	28 meteors, of which 21 proceeded from points in or near Cygnus, and 7 from Ursa Minor. Only 1 brilliant, from Deneb to α Lyræ; train visible 3 secs.	Castle Donington.	W. H. Leeson, Esq.	Communicated by Mr. Lowe.
	From 9 ^h 30 ^m to 10 ^h 30 ^m . Seven meteors; 3 between Cygnus and Delphinus 1 large, from Corona Borealis to α Hercules; another from Aquila; gold colour; direction at first S., then S.W., giving a zig-zag path.	Gosport	H. Burney, Esq.	Ibid.
13	From 9 ^h to 11 ^h . Four meteors. The first passed between Polaris and the Guards of Urs. Min.; one under and one through the tail of Urs. Maj., and the other two over Lyræ.	Ibid.....	Id.	Ibid.

Date.	Description.	Place.	Observer.	Reference.
1849.				
Aug. 13	Brightness = μ Andromedæ, with slight train; moved about 8° in 1 sec.; parallel to a line joining β with μ Andromedæ; disappeared very close to, and N. of β Andromedæ, at $20^h 17^m 8^s.08$ sidereal time, or $10^h 47^m 58^s$ Grantham mean time, taken from culminating of α^2 Capricorni.	Grantham ...	J. W. Jeans, Esq.	Communicated by Mr. Lowe.
16	$10^h 40^m$. Bright; with train; passed over the square of Pegasus.	Gosport	H. Burney, Esq.	Ibid.
	Three small, between 9^h & 11^h .	Ibid.....	Id.	Ibid.
	$10^h 10^m$. = γ ; red; tail; rapid; from χ Draconis to ϵ Urs. Maj.	Highfield Ho., Nottingham.	A. S. H. Lowe, Esq.	Ibid.
	$10^h 37^m$. = 2nd mag.; from Draco to a little above Cor. Borealis.	Oxford.....	Prof. Powell.	
	$10^h 37^m$. = $2 \times$ 1st mag.; red; train of sparks; slow; from H. 27 Ophiuchi to 52 Serpentis; train lasted 2 secs. after disappearance.	Highfield Ho., Nottingham.	E. J. Lowe, Esq.	Ibid.
	$10^h 45^m$. Small, with tail; rapid; from Vega to W. Herculis.	Ibid.....	Id.	Ibid.
	$10^h 55^m$. = 2nd mag.; rapid; from α Aquilæ to χ Antinoi.	Ibid.....	Id.	Ibid.
	$10^h 55^m$. About ten falling stars, exceedingly diminutive, but very brilliant for their size, which was scarcely = 6th mag. This gave the impression of their being fine meteors, but at a very great height.	Ibid.....	Id.	Ibid.
	One passed near Polaris, another in the Great Bear, and one in the Little Bear. All towards the W.	Ibid.....	Id.	Ibid.
	Several in Pegasus, Serpens, Ophiuchus, and Hercules; moved towards the S. Probably they all moved S.W. if they could have been seen without the effect of perspective.			
23	$10^h 28^m$. = 2nd mag.; orange-red, with streamers; rather rapid; from τ to σ Aquilæ.	Ibid.....	Id.	MS.
26	$10^h 28^m$. Small; yellow, with streamers; rapid; from ρ Delphini to τ Aquilæ.	Ibid.....	Id.	Ibid.

APPENDIX.

Containing details from the original Records of Observations, communicated to Professor Powell by the respective Authors, referred to in the foregoing Catalogue.

No. 1.—*Fall of Meteorites at Stannern, near Blansko, Moravia, Nov. 25, 1833. Note from W. W. SMYTH, Esq.*

1. On the evening in question, the appearance of a brilliantly luminous meteor was accompanied by a loud report like that of a cannon, followed by a sound like the fire of musketry. M. Reichenbach obtained information from various quarters as to the angle under which the meteor was seen, and then searched diligently with sixty or seventy men for what they supposed must have fallen, till on the 11th day they discovered a meteorite, and afterwards two smaller stones. Their external colour was black, the internal gray; the structure granular and full of metallic specks: they also attracted the magnetic needle.

2. A meteorite which fell near Stannern in May 1808, was analysed by Von Holger, and was found to contain, though in very small quantity, tin and cerium, which had not before been discovered in similar bodies. The result of five analyses was as follows:—

Silica	0.488
Protoxide of iron	0.280
Alumina	0.039
Manganese	0.085
Lime	0.068
Magnesia	0.027
	<hr/>
	0.987

The formula for the whole is $7fS^2 + 2Al S^2 + 2mg S^2 + MS^2 + 2C S^2$
 for the gray constituent $(7f + 2mg)S^2$
 for the white $(2Al + M + 2C)S^2$

Baumgartner, *Zeitschrift für Physik*, 1834, and Leonhard and Bronn, *Jahrbuch*, 1836, p. 497.

No. 2.—*Meteorite of Braunau. Note from W. W. SMYTH, Esq.*

M. Beinert of Charlottenbrunn, read before the Breslau Society an account of the fall of meteorites at Braunau in Bohemia, and exhibited plans of the locality and a portion of the iron.—*Schles. Arbeit*. 1847.

On the 14th of July, at a quarter to four A.M., the inhabitants of Braunau were roused by two violent explosions like heavy artillery, and as closely consecutive as the reports of a doubled-barreled gun, after which a rushing and hissing sound was audible for some minutes. The sky was very clear; but above the village of Hauptmannsdorf there was formed a small strip of black cloud, which suddenly seemed to grow red-hot and to dart out flashes of lightning in all directions, whilst at the same moment two fiery streaks seemed to fall to the earth. The cloud now assumed an ash-gray colour and rosette form, and after some time divided towards the N.E. and S.W., forming thin streaks which gradually disappeared.

It was soon found that the "lightning" had struck the ground near Hauptmannsdorf, about 1200 paces N.E. of Braunau, and there in a hole three feet in depth was a mass of iron which at ten A.M. was too hot to be touched with impunity. One Joseph Tepper, living in the village, had seen it fall, and gave his evidence on the subject before the authorities of Braunau. This

piece of meteoric iron, weighing 42 lbs. 3 oz. Austrian, was sent to the Imperial Cabinet at Vienna. Its form is an irregular parallelopiped, and the exterior surface is covered with concavities, the deeper parts of which exhibit a smooth yellowish brown coating.

It was soon afterwards found that the "lightning" had penetrated the dominal house called the Ziegelschlag, situated at a short distance from the town. Mr. Pollack, the chief forester, describes that he found a hole as large as the head in the roof, and a mass of broken lath and plaster in the bed-room of three children, who, when terrified by the crash, were unable to escape. The piece of iron which was found here under the ruins, weighed 30 lbs. 8 oz., and differed from the other only in form, inasmuch as it has some resemblance to a colossal oyster-shell. In breaking through the plaster, the melted surface carried off some unconsumed straw, which gives it at a distance a gold-like appearance. The chief forester Pollack calculated the height of the cloud from which the two fragments diverged at 29,351 Vienna feet = 29,562 Prussian feet*; the distance asunder of the two places where they fell being 6507 Vienna feet.

Analysis of the Braunau meteoric iron, by A. Duflos and N. W. Fischer.

Iron	91.882
Nickel	5.517
Cobalt	0.529
Copper	} 2.072
Manganese.....	
Arsenic	
Calcium.....	
Magnesium	
Silicium	
Carbon	
Chlorine.....	} 100.000
Sulphur.....	

100.000

It was afterwards found that the mass was not homogeneous, but contained portions of iron pyrites, in which Fischer found also carbon, phosphorus and chromium.—Poggendorff's *Ann.* lxxii.

Extract from a letter from W. W. Smyth, Esq.

"London, March 1, 1849.

"I have just met with a curious fact, viz. the presence of *phosphorus* in certain meteoric irons.

"Berzelius found in the meteoric iron of Bohumilitz certain steel-gray laminettes and grains, which he proved to be composed of iron, nickel and phosphorus. Lately, my friend Patera at Vienna has analysed a similar mineral in the meteoric iron of Arva. It was observed in small leaflets, which are flexible, and have a strong effect on the needle; the hardness = 6.5, the spec. gr. = 7.01 to 7.22, and the composition—

Phosphorus	7.26
Iron	87.20
Nickel	4.24
	98.70

98.70

"The mean of three analyses also gave a small quantity of carbon. The name *Schreibersite* has been proposed for this new mineral.

"Yours ever,

"WARINGTON W. SMYTH."

* Above 30,000 English feet, or five miles and five furlongs.

No. 3.—Letter from Dr. Buist to Professor Baden Powell, Oxford.

"Bombay, July 22, 1849.

"Dear Sir,—I now enclose some notices of those meteors of lesser magnitude and greater frequency noticed at Aden, by Mr. Moyes in 1843, and by my assistants while in charge of the observatory here, in 1843 and 1844.

"I am at present in communication with observers at thirty different stations scattered over India, from latitude 10° to 33° , and am making arrangements to get returns from every spot where an European is stationed in the service of government. By these means, I shall, I hope, have it in my power to furnish you with a long and minute catalogue of meteors every year. Careful simultaneous observations along chains of stations will soon come to give us the relation of different meteors to the stars and constellations they seem to approach or traverse, and furnish us with the elements of computing their size and distance. The newspapers I have already sent will have given you all the information I possess in reference to the larger meteors; in the "Times" now forwarded, is a description of one seen at Kurrachee on the 25th of June (our sky at this season is covered with clouds), which, like that of the 19th of March, was *heard* to explode.

"The leading characteristics which distinguish our larger and lesser aërolites are the following:—the larger generally appear as luminous as a star of the first, the lesser scarcely so much as a star of the third magnitude.

"The light of the larger meteors is generally orange, bluish or greenish, hardly ever white. It resembles that of a star of a Roman candle, as if given out by a considerable mass of matter: it never exhibits rays like a fixed star or the light from electricity; it is never at all dazzling. The meteor always seems to increase in velocity and bulk as it proceeds in its path, the result probably of perspective; and when approaching the termination of its course, it commonly flames out with unusual brilliancy; there are about as many which disappear at once, as if extinguished, as those which burst and fall in fragments. The fragments always cease to be visible at some 5° to 15° from the ground. The only meteors that have been heard to explode this season were those of the 19th of March, heard at Aurungabad, and 25th of June, heard at Kurrachee.

"From the 1st of June to the 1st of September our sky is thick and cloudy.

"If meteors fall over the twenty-four hours indiscriminately, the number entering our atmosphere must be immense. They are not visible till after sunset, and by eight or nine o'clock we are all indoors, by ten we are in bed; two hours thus is all the time allowed for observation. We expect to derive the greatest advantages from the services of European sentries on duty, as we are now striving, with every hope of success, to engage the army in our service.

"Our November meteors cross the sky in all directions: they very much resemble fire-flies, only they are much more swift and rectilineal in their movements. They do not alter either in apparent speed or size as they proceed; they never flame out or appear to burst; they very rarely approach the horizon, and having traversed ten or twenty degrees of space, become lost in darkness.

"I ever am,

"Most faithfully yours,

"GEORGE BUIST."

No. 4.—From the Bombay Times, November 1, 1847.

"On the 7th of September, about half-past six P.M., a large fire-ball was seen at Poona to shoot from nearly north to south: it then made a sudden sweep, and proceeded nearly at right angles to its previous path. After being visible for five or six seconds, it split into a number of large fragments, which

rapidly descended towards the earth; and these again broke up into lesser fragments, till they appeared to descend in a shower of sparks. Before the first bursting, the meteor was of exceeding brightness, of an intense blue colour, and at the instant of explosion it changed into red:—it seemed to light up the whole heavens, though the moon was shining, so as to render the lesser stars visible.”

From the *Bombay Times*, November 1, 1847.

“On Sunday evening (Oct. 30), about seven o'clock, a magnificent fire-ball was seen to shoot across the air from nearly west to east, when its horizontal motion suddenly ceased and it seemed to drop perpendicularly into the sea betwixt Mazagon and Sewree. At the time of its explosion—for such we may take that of its change of direction to have been—its illuminating power was equivalent to that of an ordinary-sized blue light: it dazzled the eyes of those near it and who looked at it directly; and though the evening was at the time perfectly dark, the most minute objects in the landscape were for ten or fifteen minutes made visible by it. It appeared to become extinguished some three or four hundred feet before touching the water. It left a long train of light behind it, which was visible for the space of nearly half a minute.”

No. 5.—From the *Bombay Monthly Times*, June 18, 1848.

“At a meeting of the *Bombay Geographical Society*, the following letter was received from Captain George Wingate, of the *Bombay Engineers*:—

“I beg to transmit two fragments of an *aërolite*, which fell about one o'clock P.M. of the 15th of February last, 1848, in a field to the south of Negloor, a village situated within a few miles of the junction of the Wurda and Toombodra rivers, and belonging to the Gootul division of the Ranee-Bednoor talook of the Dharwar collectorate.

“The fall of this *aërolite* is most satisfactorily established. A cultivator of Negloor, named Ninga, was driving his cattle out to graze close by where it fell, at the hour above mentioned, when he suddenly heard a loud whirring rushing noise in the air, but on looking up could see nothing. An instant afterwards, however, he observed a cloud of dust rise from a spot in an adjoining field, as if something had struck the ground there with violence. At this time several other villagers were standing by a threshing-floor close at hand, who also heard the noise, and one of them called out to Ninga asking whether he had also done so. He replied, Yes, and that something seemed to have fallen in the next field, where he saw the dust rise, pointing at the same time to the spot. The whole party then immediately proceeded there, and found to their astonishment the *aërolite* broken into fragments, of which those now forwarded were alone of any considerable size. The stone, from the velocity of its descent, had made a hole of several inches in depth,—like the print of the foot of a young elephant, as the villagers described it. They were naturally much puzzled to account for the appearance of the stone, which altogether differed from any to be met with in their neighbourhood; but at length were constrained to conclude it had fallen from the sky. The circumstance seemed so extraordinary that one of them was immediately sent to summon the Patel of the village to the spot, who soon arrived, attended by a crowd of people who had also heard the wonderful tidings. These too unanimously adopted the same conclusion regarding the fall of the stone, and the Patel took into his charge the accompanying fragments, and wrote a report of the whole circumstances to the Mahalkurree of Gootul, who is revenue and police officer of the district in which Negloor is situated.

“ ‘The Mahalkurree thought the Patel’s report so extraordinary that he determined at once to proceed to Negloor himself, to inquire as to its truth, which he did; and after having examined the stone itself, as well as the hole in the ground made by its fall, and found all the accounts of the villagers who were present to agree, he could not avoid coming to the same conclusion that they did, regarding its fall from the sky. To place the matter beyond doubt, however, he took statements in writing of the circumstances from the cultivator Ninga and another, who had heard the rushing noise made by the stone in its passage through the air, and forwarded their depositions, with his own report and the fragments of the *ærolite*, to Mr. Goldfinch, the assistant collector and magistrate in charge of the district, who has kindly placed them at my disposal.

“ ‘Had the evidence in proof of the fall of this stone been less conclusive than it is, we might still have inferred the fact of its being an *ærolite* from its peculiar appearance, so different from that of any rock in the neighbourhood of the spot where it was found. For miles around the village of Negloor, the only rocks to be found are primary clay-slate of various degrees of induration, and occasional dykes, masses and boulders of greenstone, but not a trace of any volcanic product, or other stone bearing the remotest resemblance to the one under consideration. The latter, moreover, tallies exactly with the descriptions given of *ærolites*. It is coated with the fused crust or film characteristic of these bodies, and is evidently highly metallic. On the theory of *ærolites* being planetary bodies which become fused on their surfaces, and burst by the sudden evolution of heat occasioned by their rushing at immense velocities into our atmosphere, the specimen now forwarded may be supposed to have formed part of a globe, or rather a mass approaching the spherical shape, of somewhat more than a foot in diameter, which burst into fragments under these circumstances; and the difference in appearance of the position of the fused film over the rounded part of the specimen, which may be considered to be a portion of the surface of the original globe, and of that coating the remaining parts, which according to this view were the rough broken surfaces of the detached fragment, would seem to favour this explanation.

“ ‘These remarks, however, are merely thrown out in the way of conjecture, as I do not pretend to any knowledge that would entitle me to theorize on the subject at all. My object in writing at so much length has been to show that the specimen now sent is a part of a true *ærolite*, and as such, I hope it will be thought worthy of a place in the new Museum.’

“ ‘The mass of stone which accompanied this was somewhat ovoidal: it weighed four pounds, measuring fifteen inches round the larger, and eleven round the shorter axis. It was covered over with a black-looking vitrified crust about one-twentieth of an inch in thickness. This refused to yield to the action of muriatic, nitric, or sulphuric acids. One end of it was marked with impressions such as a slightly softened body might receive on being thrown violently against the earth. The specific gravity of the crust was a little over three, or somewhat heavier than marble; it had not been quite accurately determined, from the difficulty of separating the crust from the interior. The interior of the *ærolite* was exactly like softish white sandstone; it crushed between the fingers, and absorbed, when immersed an hour in water, one-hundredth of its weight. Its specific gravity was 3·5, or a third heavier than the heaviest sandstone, that of quartz being 2·6. It slightly effervesced with muriatic acid, giving off much sulphuretted hydrogen gas, and then slowly dissolved into a glutinous mass. It seemed full of metallic particles, which shone beautifully under a moderate magnifying power, with

a dull light. The following note by Dr. Giraud gives particulars of the results of the first examination of its characters:—

“The stone is acted on by cold hydrochloric acid, with disengagement of sulphuretted hydrogen. Boiling, but not cold, nitric acid acts on it violently, disengaging HS and NO₄. The great part of the stone is silica: the metallic granules consist of iron in equal proportions, with nickel and chrome—in fact meteoric iron. The nickel of course is much obscured by the iron: the chromine was readily detected, for on fusing the stone with nitre, dissolving the fluid mass in distilled water, and then testing with acetate of lead, a fine yellow chromate of lead was obtained. On fusing the stone with nitre, chromate of potash was of course produced. I cannot detect any cobalt, which you know Stenmeyer found in the mixture of iron at the Cape of Good Hope.”

No. 6.—Letter from E. J. Lowe, Esq. to Prof. Powell, enclosing one from Sir J. W. Herschel, &c., received July 11, 1849.

“My dear Sir,—The following account of a meteor was sent me by Sir John Herschel; I accidentally omitted to forward the account of it to you with my former catalogue. I may remark that it was noticed at Bath, but am afraid by no one capable of accurately describing it; perhaps the insertion in the British Association Catalogue may be a means of obtaining further information of this fine meteor.

“Yours ever truly,

“E. J. LOWE.”

“My dear Sir,—The parents of a young person residing in our family (of the name of Atkins) were aroused on the night of March 8th, by a noise, which induced Mr. A. to get up. At four A.M. he was struck with a great light; it emanated from a meteor larger than the moon which shot across *above Windsor Castle* as seen from Slough (two miles), *i. e.* looking nearly southwards. I can get no correct notice of the altitude above the horizon, which is a pity, as it seems to have been a first-rate one, and its course being horizontal and from west to east, must have been seen on the French coast, and probably by seafaring people (who watch nightly) in the Channel. I enclose a note of explanation from Mrs. A. to her daughter; perhaps you may have some corresponding observations, in which case it will be worth while to question further about the apparent altitude as seen from Slough.

“Your very faithful Servant,

“J. F. W. HERSCHEL.”

Extract from a letter received by Sir John Herschel on the meteor, from Mrs. Atkins.

“On this morning (March 8, 1848), four A.M., a large body of light in the shape of a kite, more brilliant and larger than the moon, passed across from west to east; it moved gently; indeed your father had time to wake me, and I to get up to the window before it disappeared; the colour was a strong blaze of fire; it shot from the clouds and disappeared in the same. It travelled from the west of the Castle to the extremity of Datchet. The stars were shining at the time. The noise that awoke Mr. Atkins, had not, in his opinion, anything to do with the appearance. It was of this shape.”



No. 7.—Extract from a letter dated Pisa, Tuscany, Sept. 9th, 1848.

“... Last evening, Sept. 8th, about ten minutes before seven, I observed from my window, facing due south, a luminous ball of fire, about the size of an orange, glide gently past from N.W. to S.E. The moon was up; it passed

under the moon, and seemed to spend itself before it would otherwise become invisible from the convexity of the earth. Mrs. Irving also saw it, but I am not aware of any one else.

“I have the honour to be, Sir,

“Your obedient Servant,

“To Professor Powell, Oxford.”

“JAMES IRVING.”

No. 8.—Extract from a memorandum communicated to Prof. Powell by W. H. Black, Esq. of the Rolls' Office, dated Mill-yard, Whitechapel, London, March 6, 1849.

“This evening (March 6, 1849), soon after sunset, a bright meteor fell. It began its path somewhat below and to the southward of the ζ , and fell in a curve, brighter all the way than η (which was then shining in the west), and exploded at the end of that curve with a flash, its body appearing of the colour and brightness of the ζ , somewhat lanceolated, half as large as the ζ , and slightly greenish and red in the flash with which it expired or disappeared.

“It was about 1 second or $1\frac{1}{2}$ in falling; and the time was (as nearly as I could ascertain by my watch and clock) $18^h 8^m$ C.T., or $6^o 8'$ P.M.

“The window from which I saw this phenomenon looks directly eastward; and as I stood on the left side of the window, I could clearly see the S.E., and marked the exact spot where it disappeared, as well as its path through the leafless boughs of a tree, the ζ being about 45° in height, S.E.E.; and the meteor exploding about 12° or 15° in height S.E. from me.”

No. 9.—The Bombay Times, March 21, 1849, gives a statement from a correspondent, announcing the appearance of a luminous meteor at Bombay, on Monday, 19th March, at $6\frac{1}{2}$ P.M.

Ibid. March 24. The editor adds:—“The meteor, as seen from the esplanade, seemed to issue from a thin streak of cloud overhanging the dock-yard. It thence rushed in a north-easterly direction, as if over the custom-house and towards the town-hall. The light it emitted was of a brilliant green: when it exploded it seemed resolved into a mass of red embers. The meteor was seen from Poona, Tannah, and probably over a very large expanse in the interior, and must have been, when it exploded, very much higher in the air than it appeared.”

The following are extracts from correspondence subjoined:—

“I (with others) was to the north-east of the police hulk on the evening in question, and saw the fire-ball, which appeared to rise from one of the ships lying nearest to Mazagon: this brilliant meteor might have been at any distance you please in the N.E., though we fancied that it was within three hundred yards of us.—F.”

“On Monday (19th) evening, as I was taking a walk with a friend of mine on the Grant road ‘flats,’ my attention was attracted to, as it were, a planet of the size of a common-sized hen’s egg(?). A second or two did not elapse from the time I saw it whole till it burst, and the light that it shed was unusually brilliant for a meteor. I may here mention that *we* were not the only persons who saw it; for on my going to the fort the next morning, a friend of mine told me that as he was spending the evening at Mazagon, he saw just what I have related.—G.”

“On Monday the 19th inst., a meteor answering the description was seen by a friend of mine about the same hour on that evening in a N.E. direction. It was first seen in the form of a ball about the size of a large egg(?), darting

towards the earth: it broke into numerous small brilliant fragments, and disappeared. It was visible about half a minute.—S.”

Poona, 22nd March.

In the same journal of March 28, further particulars are given, of which the following are extracts:—

“Subjoined is a series of notices of the luminous meteor seen on the evening of the 19th, which now appears to have been a much more magnificent variety of aërolite than was at first supposed. It appears to have been at a great elevation, and, as suggested by a Poona correspondent, was probably some hundreds of miles from the nearest spectator when first seen. The volume of the mass, the length of its course, and the velocity with which it rushed along, may from this be imagined. As formerly observed, when first seen at Bombay it appeared as if nearly over the dockyard: in this all the observers who noticed it in different parts of the island concur. Curiously enough, we have not been favoured with a single notice of it from any one on board the ships in the harbour: from the anchorage we have no doubt it would also appear to the eastward. At Poona—lat. $18^{\circ} 30' N.$, long. $72^{\circ} 2' E.$ —it was observed at a quarter past six at the altitude of about 30° : it was visible from Poorundhur, twenty-six miles east of Poona. It was observed at Aurungabad, lat. $19^{\circ} 45' N.$, long. $75^{\circ} 30' E.$, as if to the south; and from Sholapore, lat. $17^{\circ} 40' N.$, long. $76^{\circ} E.$, where its appearance was most distinctly observed, and has been most carefully described, as seen in a north-easterly direction. It was seen at Jaulnah, particulars not given. It was also carefully observed at Surat, $21^{\circ} 11' N.$, $73^{\circ} 7' E.$ It has thus been described as visible over an area of above 3° of longitude and 2° of latitude—from Bombay $18^{\circ} 53' N.$, and $72^{\circ} 49' E.$, to Sholapore and Aurungabad; though in all likelihood it may have been observed over a much more extensive area than this, from which as yet no observations have reached us. From the explosions heard at Aurungabad, it is possible that in this neighbourhood it burst.

“Another meteor, of lesser size, though still of considerable brilliancy, was seen here on the 23rd.”

“*To the Editor of the Bombay Times.*”

“Sir,—The meteor alluded to in your last was seen at this place on Monday, at six P.M., in the E.N.E. quarter, at an altitude of about 30° ; descending obliquely towards the E., it disappeared behind a building. I had only a glimpse of it myself, my back being towards it at first, but the person with me described it as being about half the size of the moon, and much brighter. It was seen also at Poorundhur. Instead, therefore, of being a few hundred yards distant from your Bombay informant, it must have been certainly more than 100 miles: how much more we cannot say, not having its exact bearings, but probably another fifty at least. Should it have been seen at Seroor or Nuggur, something further may be known of it, as it must have been pretty near those places. If I can collect anything further about it I will let you know.—W. S. J.”

“Poona, 23rd March, 1849.”

“On Monday (19th), about half-past six in the evening, a very remarkable and beautiful meteor was observed at this station. Its course was north-easterly; bursting out in the zenith in a most brilliant manner. It appeared to me to have two succeeding periods of intense brilliancy, with intermediate diminutions of light ones; occurring on its passage behind a fleecy cloud near the horizon, it illuminated it like a sheet of summer lightning would, the colour then assuming a vivid green hue. I have never seen any so beautiful and striking that I remember as this.—OBSERVER.”

“Sholapore, 25th March.”

"SIR,—One of your correspondents I see has sent you an account of a fire-ball which he saw on Monday (19th) evening last at about half-past six o'clock. The same meteor was visible here at Surat; and so much did its extreme brilliancy and very rapid motion give the appearance of nearness to it, that we thought it must have fallen close to the town. Of course to speak by the card, we ought to say that at about half-past six we saw a meteor which, &c. &c., but you will see from what follows, that it is not a very rash inference to conclude it to be the same. If you can get from some of your friends an estimate of its apparent altitude, and the direction of its motion as seen at Bombay, the data sent with this will perhaps help you to a rough approximation to its real height, volume, and path. It passed then from west to east across our meridian to the south of us; was about 30° high when first seen, and perhaps 10° when it vanished (25° perhaps when on the meridian), and must have had an apparent diameter of 4' or 5' (say for the sake of comparison with other estimates, about one-eighth of the diameter of the moon). It was intensely incandescent, the surface appearing as if liquid with heat (having so large a diameter, one seemed to be able to look well into the surface); in colour white, with perhaps a slight tinge of green. (Query—the optical effect of contrast with an evening sky?)—H."

"P.S.—A friend accustomed to estimate angular magnitudes, and who saw the meteor, confirms the account I here give you, but adds that he 'saw the train distinctly visible about 5° higher than Canopus.' This, from the position of Canopus at the time, would give the meridian altitude about 21° instead of 25° ."

"Surat, 24th March."

"Sir,—A meteor of the same description as the one seen in Bombay on the evening of the 19th, was also seen at Jaulna at the same hour.—G. F."

"Camp, Jaulna, March 24."

"Sir,—The meteor noticed by your correspondent in your last issue was also observed by several persons at this station on the same evening, and about the same hour (Monday the 19th, half-past six o'clock). It seemed to arise a little to the south of, and above Venus, and to travel in a northerly direction; I should say N.N.W. About a minute and a half after it disappeared, two reports, following each other rapidly, were heard, like the explosion of a mine at a considerable distance.—ASTERCA."

"Aurungabad, 24th March, 1849."

I have been also favoured with a sight of a private letter from an astronomical friend to the Editor of the Bombay Times, of which I am permitted to give the following extract:—

"Poona, 2nd April, 1849."

"..... Can you make anything out of the different reports of the meteor of the 19th ult., so as to have even a guess at its whereabouts? I cannot by any means *torture* them into an agreement, and have come to the conclusion that there must have been *two* at intervals of perhaps fifteen or twenty minutes, and that they have been confounded together; *e.g.* how could the same object have passed the south meridian at Surat, at an altitude of 21° from W. to E., and also have burst out in the zenith of Sholapoor and moved N.E.? The latter could have been seen only to the east of the meridian at Surat. Also if the interval between the appearance and report at Aurungabad is worth anything, the distance from that place could not have been much more than twenty miles, and this does not seem to tally well with either of the other two. I had no watch about me to note the exact time here, but it was only a few minutes after sunset, certainly not so late as a quarter-past six, while at Surat the difference of longitude must have made it a trifle earlier: so that the times do not agree very well either."

No. 10.—Bombay Bi-monthly Times, April 30, 1849.

"A correspondent mentions that about half-past six on the evening of the 26th of March, a meteor of considerable magnitude was seen from Cochin, travelling in a north-westerly direction. At first it seemed somewhat larger than the planet Venus, as now visible: it consisted of a nucleus of bright emerald green, with a long tail of an uniform red colour. It burst into fragments as it approached the earth. Our informant was not aware of any report having been heard accompanying the explosion."

No. 11.—Bombay Bi-monthly Times, April 30, 1849.

"The meteor of the 13th April (Friday).—A writer in the 'Poona Chronicle' gives us a notice of the meteor seen at Hingolee and Bombay on the 13th April, and completes the chain of evidence, establishing the fact that it was the same body which was visible at all the three points. None of the observers speak of its explosion, so we are left to infer that it continued to travel eastward beyond the reach of vision. It seems to have proceeded from west south-easterly—the Poona observer having obviously first seen it after it had passed him, so as to make it appear in the east: proceeding further easterly, Hingolee is in lat. 77° E., or nearly—Bombay $72^{\circ} 49'$. At the former place it was seen at nine very nearly, at the latter at from twelve to fifteen minutes after nine. If it travelled at the rate of thirty miles a second—the supposed velocity of the meteor of the 19th of March—it would occupy ten seconds from Bombay to Hingolee, assuming the distance to be 300 miles: this is an amount of time that need not for the present be taken account of; and it may be assumed to have been seen at the two points simultaneously. The difference of time due to longitude, taking this at four degrees, would be sixteen minutes; and this corresponds very closely with the observed difference. The Poona writer, who says he saw it three minutes before nine, is obviously wrong—it must have been twelve or thirteen minutes after, he means."

Ibid. From a correspondent:—

"..... Last night as a friend and I were seated in a "Chubooturah," in front of my house, enjoying a refreshing zephyr that had just sprung up after a day of intense heat, and as I was contemplating the blue and spangled vault over our heads, my attention was attracted to a beautiful meteor, to which I immediately drew my friend's attention. The time, just as the evening gun had sullenly boomed at nine P.M., and its echo had scarce finished reverberating among the adjacent hills, when this body burst into view a little to the west and south, just as if the concussion had broken a portion from off one of the spheres above, and what we saw was the falling debris. From where we were seated, the apparent nucleus, whence it started, seemed not to be more than twenty-eight or thirty degrees in height from the to us then visible horizon. It left behind it a train of most beautiful light, and which appeared to us by no means inconsiderable in breadth—colour that of a most beautiful 'blue light.' The coruscation lasted for several seconds, when I lost sight of it behind my office bungalow.—J. J. H."

"Hingolee, April 14th, 1849."

"* * * An account of a meteor of the same description exactly, seen at Bombay at the same hour of the same evening, appeared in our last. If it was the same it has travelled over nearly 300 miles of country from E. to W.—EDITOR."

No. 12.—Bombay Bi-monthly Times, May 11, 1849.

"Poona, 2nd May, 1849.

"On Monday, 30th April, at 7.7, a meteor was seen just under β Ursæ

Minoris, descending obliquely to the left at an angle of about 55° . It disappeared at an altitude of about 6° , at which time its azimuth must have been $N. 10^\circ$ or $11^\circ E.$ Its whole visible tract did not exceed 7° or 8° , and it was brightest just before disappearing, but did not then exceed a star of 2nd magnitude. Its colour was dusky red; duration perhaps $1\frac{1}{2}$ second.—W. S. J.”

Ibid. “Subjoined is an interesting notice of the meteor mentioned in our last. May not the circumstance alluded to by our correspondent account for the showers of dust and ashes occasionally observed at such vast distances from volcanos as to have proved subjects of much perplexity to those trying to explain them on the assumption of this being due to eruptions? The appearance of red snow, showers of blood, and the like, would be at once produced were iron reduced to peroxide by combustion to commingle with snow or rain during their fall.

“On Thursday* evening, at Malabar Point, half an hour after sunset, I observed a splendid meteor about S.S.W., falling slowly on a plane inclined at an angle of 80° to the horizon (the acute angle being to the right). Its angular velocity about 2° per second. Mean time of observation $6:52\frac{1}{2}$ P.M. It passed about 2° to the left of a star of the first magnitude (viz. Canopus, its true altitude being $6^\circ 53'$, and true bearing $S. 27^\circ 28' W.$). When first noticed it was a few degrees above the star, shining with a steady planetary light like Jupiter. When it had fallen to about the same altitude as the star, it blazed out with an intensely dazzling white light, brighter than Venus; then quickly faded into a shower of what appeared dull reddish yellow sparks, and ended its course in a vertical direction, disappearing when about 2° above the horizon. If this was an extra-terrestrial body, the direction of its motion in space showed as if it had overtaken the earth or its orbit. If the blazing out occurred when entering the atmosphere, its distance from Bombay must have been considerable (probably 150 miles), and its size corresponding (perhaps thirty yards in diameter). It would be interesting if simultaneous observations could determine the height of this blazing appearance that almost all meteors have at some part of their course. I forget if Humboldt has anything conclusive on the subject. One remark, *not to be found in the ‘Cosmos,’* but nevertheless true, is, that if a 32 lb. iron shot played the part of an aërolite and entered the atmosphere with the velocity of ten miles per second (a moderate velocity for such bodies), the heat generated by the resistance of the atmosphere would be sufficient to raise the temperature of the shot one million of degrees. Such an immense and sudden evolution of heat would probably not only melt the iron, but oxidize it with an exhibition of intense combustion, and the splendid meteor would finish its course by gently descending to the ground in the shape of an insignificant red powder. Has this been the fate of the young planet of the 19th March?”

“The meteor was seen at Khandalla a few minutes before seven o’clock, travelling from N.W. to S.E., and is described as having been of exceeding brilliancy.

“The following extract is from a letter received a month since from Hoshungabad, dated 6th April: can it be that the thick dust which filled the air betwixt the 28th March and 2nd April, was the debris of the meteor of the 19th (March)?

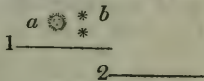
“From the 28th of March to the 2nd of this month, a haze, occasionally very dense, has covered the station. The opposite hills were sometimes invisible, and the sun could be viewed with the unprotected eye until he attained an

* The day of the month does not appear, it was probably *after* the meteor of Friday 13th, and before the 30th: either 19th or 26th April.

altitude of 30° or 35° , having all the appearance of a disc of silver, and the same transpired (?) when about an equal number of degrees above the western horizon. On these days an impalpable dust fell and covered all things. Not a cloud was to be seen, but this uniform and general haze mantled everything day and night,—the moon and some stars of the first magnitude only being visible at night. Barometer slightly lower, dew-point steady, electricity abundant, and a gentle breeze chiefly from the west. The days felt oppressive, and notwithstanding that the haze intercepted the full play of the solar rays, the black bulb thermometer reached 120° on the 30th."

No. 13.—Extract from a note to Prof. Powell from the Rev. J. Slatter, dated Rose-hill, August 7, 1849.

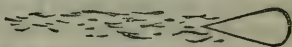
"The observation would seem to favour Sir J. Lubbock's theory of meteors shining by reflexion, but for a concomitant circumstance; just before it expired it threw off as it were some bright particles like the snuff of a candle, which fell slowly downwards beside it, and were not extinguished till they had fallen below the point at which the meteor ceased to be visible: thus, *a* ceased at line 1, *b* at line 2. No other impression I think could be left on any observer's mind, but that it was matter in a high state of incandescence.



"It was to appearance five or six times the size of Vega, and intensely bright. I should think from the hour and the fineness of the evening, it will have been seen by other observers. It had much the appearance of a fine ball from a Roman candle."

No. 14.—Extract from Mr. Lowe's communication to Prof. Powell.

"Aug. 8, 1849, $10^h 16^m$. A splendid meteor, more than twice the size of a first magnitude star, of a conical shape, moved very slowly horizontally from ξ Bootis, passing 1° below Arcturus with numerous stars left behind; here it vanished, but in about 1^{sec} . reappeared about $1\frac{1}{2}^{\circ}$ farther on, it having moved onwards in the same tract, but invisible until it had gone over $1\frac{1}{2}^{\circ}$ in space; it remained visible about 5^{sec} . altogether independent of the second of time it was invisible. After its second reappearance it was not so brilliant as when first noticed; indeed it had the appearance of moving rapidly from us; and if we suppose it was moving nearly directly away from us, it would have the appearance of gliding slowly amongst the stars. At the second apparition it made a continuation of its former track 3° in length; its colour was orange-red.—E. J. L."



No. 15.—Note from Dr. Hopkins of Birmingham:—

"On Monday, Feb. 15, 1830, walking from Edgbaston to Birmingham, I was startled by the appearance of a brilliant light in the sky, and looking up, for my eyes had been turned to the ground, I perceived a bright mass moving in a direction from N.E. to S.W. The size of the body appeared nearly that of the full moon. It remained visible about two seconds, moving very rapidly, then nearly disappeared for a moment, and after being visible about two seconds more, suddenly vanished. It left behind it a marked trail of light, which was very distinctly visible for a short time after the disappearance of the mass. It seemed to have rather a waving motion, but this appearance was probably owing to the thickness of the fog, which rendered the light much less brilliant than it would otherwise have been; as it was, the houses and other objects were rendered much more distinctly visible than they

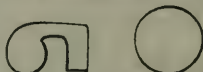
would have been by the light of the full moon. The exact situation from which I had a view of this interesting object was about twenty yards farther from Birmingham than the Plough and Harrow public house at Edgbaston; and the time was, as nearly as I can tell, about ten minutes past seven."

No. 16.—From a letter to the Rev. Prof. Powell:—

"Birmingham, Sept. 13, 1849.

"Rev. Sir,—I furnish you with a written account of what I suppose to be a meteoric appearance, which I saw some years ago at Palamcottah, in South India. I am unable to lay my hand at present upon a brief memorandum which I believe I made at the time, and therefore cannot furnish the date more accurately than to say it was in the year 1838.

"At about half-past seven o'clock in the evening, two young men living in a house thirty or forty yards from mine, were taking their tea together, with their doors and windows all open, as is usual in India, when their attention was suddenly attracted to a bright light shining outside, which at first they took to be moonlight; but remembering that there was no moon at that time, they went outside to see what it could be. They beheld on looking up a brilliant object in the heavens, shining more brightly than the moon, and instantly came and called me to see it. By the time I had reached the outside of my house, its brilliance had considerably faded, but even then it was a glorious object. Its position was directly north, its elevation about forty-five degrees, perhaps a little higher; its form I well remember, because of its resemblance to a letter in the Tamul alphabet, and its whole surface, though different in shape, little less than that of the moon. Its shape and relative size to the moon may be represented thus. What appear to me to be its great peculiarities were these: it was perfectly stationary, never moving for a moment from the place where it was first seen: and it remained visible twenty minutes from the time I first saw it, becoming more and more dull and indistinct, till it melted away and was seen no more. I should add that it was a starlight night, without a single cloud.



"I have the honour to be, Sir,

"Your obedient Servant,

"G. PETTITT."

No. 17.—*General Results of Observations on Meteors.* By EDWARD JOSEPH LOWE, Esq., F.R.A.S.

(1.) Periodicity of meteors.

The following epochs are known as periods when falling stars are abundant.

April 22–25, July 17–26, August 9–11, November 12–14, November 27–29, December 6–12. To this number I add October 16–18.

I have found the month of January frequently to have a brilliant display of meteors, but the day is not stationary. In 1844 they were abundant on the 26th; 1845, on the 31st; 1847, on the 11th and 13th; and 1848, on the 4th.

The annexed shows when falling stars have been numerous in the various epochs since 1841, and when and by whom observed.

April epoch 22,–25.					
1848	^a 23	on the Clyde by Mr. Symonds.
				Highfield House.	the Author.
1849	20	id. id.
	26	id. id.

The greater number this year occurred on the 20th; the April period has become rich in its display of meteors in the last two years.

July epoch, 17-26.

1846	^a 25	Highfield House..	the Author.
		30		id.	id.
1849	20		id.	id.
		21		id.	id.
		23		id.	id.
		24		id.	id.
		26		id.	id.
		27		id.	id.

This epoch was very meagre until the present year.

August epoch, 9-11.

1841	^a 10	Plymouth	Prof. Phillips.
		9	Greenwich	Mr. Hind.
1842	9	Gosport	Mr. Maverly.
		9	Greenwich	Mr. Hind.
1843	9-13.		Cork	Prof. Phillips.
		10	Highfield House..	the Author.
1844	10	Durham	Mr. Wharton.
		10	Greenwich	Mr. Breen, jun.
1845	10	Paris	M. Gravier.
		11	Greenwich	Mr. Breen, jun.
		10	Oxford	Prof. Powell.
1846	10	Dijon	M. Perrey.
		12	Greenwich	Mr. Breen, jun.
1847	10	Durham	Mr. Wharton.
		10	Oxford	Prof. Powell.
1848	10	Highfield House..	the Author.
1849	10	id.	id.
		10	London	Mr. Birt.
		10	Gosport	Mr. Burney.

The August epoch rarely ever fails to bring a very abundant display of meteors. In 1841 Mr. Hind counted at Greenwich seventy-two meteors, between 10^h and 15^h; the greatest number in one hour was from 12^h to 13^h, viz. 24. In 1842, Mr. Hind saw 100 between the 11^h and 16^h. The greatest number in one hour, from 13^h to 14^h, viz. 20. This year (1849), about eighty were counted here in an hour, from 10^h to 11^h. The meteors for a few evenings previous to the 10th, when the sky was in a condition for falling stars to be seen, gave an increase in number each evening. The 10th was only clear for an hour, viz. from 10^h to 11^h.

October epoch, 16-18.

1843	^a 16	Highfield House..	the Author.
1844	18		id.	id.
1846	16		id.	id.
		17	Dijon	M. Perrey.
1847	18	Paris	M. Laisnè.
1848	18	Highfield House..	the Author.

This epoch, which has returned so regularly from 1843, I have not seen entered as a period for falling stars.

1st November epoch, 12-14.

1841	^a 12	Greenwich	Mr. Glaisher and Mr. Dunkin.
1843	11		id.	Mr. Hind and Mr. Paul.

1844	^d 12, 13..	Birmingham	Mr. Onión.
	12, 13..	Highfield House..	the Author.
1845	10	Greenwich	Mr. Lovelace and Mr. Breen, jun.
		14	Bombay	Prof. Orlebar.
1846	11	Greenwich	Mr. Humphreys, Mr Love- lace, and Mr. Breen, jun.
1847	12, 13..	Dryburn	Mr. Wharton.
		12, 13..	Highfield House..	the Author.
		12, 13..	Benares	Correspondent to M. Arago.

Although this period in former years exceeded all others, still within the last few years the August epoch has been more brilliant.

2nd November epoch, 27–29.

As yet I have never been fortunate enough to see a meteor on these nights.

^d December epoch, 6–12.

1845 12 Bombay

Prof. Orlebar.

1847 12 Highfield House..

the Author.

(2.) Meteors crossing the sun as dark spots.

In 1839, in *Astronomische Nachrichten*, No. 385, Prof. Erman stated that the cold days of the 11th to the 13th of May and the 5th to the 7th of February, were owing to the passage of falling stars between us and the sun.

In 1845, the German astronomer Peters had taken observations on solar spots, which he requested the Astronomer Royal Capocci to have continued. One of the assistants (M. de Gasparis) being thus occupied on the 11th of May 1845, observed a black body cross the sun; he called Capocci's attention to the fact, and he, together with Dr. Demartini and an assistant astronomer (Del Re), witnessed great numbers cross his disc. Being on a visit with my worthy friend Mr. Lawson at Bath (this year), we determined to watch carefully for these black globules, and accordingly set the 11-foot equatoreal to the focus of the sun; a 5-foot to that of Venus; a 3-foot to that of the moon; and a beautiful defining glass of thirty inches, to 400 miles, thinking by this arrangement to be enabled to detect these bodies, whatever distance they might be from us; unfortunately, from February 5th to 7th, the sun never shone at Bath; however, we received a communication from Mr. Weeks of Sandwich, saying his friend the Rev. Mr. Brown of Deal had seen two deliberately cross the sun's disc in a descending arc. We looked out again on the 11th, 12th and 13th of May, but without success. Conversing with my friend Mr. Hind, he informed me that Messier* remarked, that in 1777 one day he had seen 200 small dark bodies cross the solar disc; to say the least of the phenomenon, it is worthy of a few years' attention before we decide this interesting point, especially as the November meteors frequently fail to give us a rich display.

(3.) On a point of divergence of meteors.

The meteors seen in 1839 diverged from a point situated between Taurus and Pegasus; since then the point is stated to be near β Camelopardalis. Both last year (1848) and this (1849), from a great number of observations, the point of divergence was in or slightly above Cassiopeia on the 9th to the 18th of August, yet, strange to say, until then this point was not observed: there was another situated in Cygnus, which had been plainly discernible since the middle of July. From that time until the 9th of August, if the paths of

* Messier gave a memoir on the subject, entitled, "Observation singulier d'une prodigieuse quantité de petites globules qui ont passé au devant du disque du soleil." [Mem. Acad. Paris, 1777, p. 464.]

the meteors were produced backwards they would nearly all meet at a point situated to the east of *Alpha* Cygni, and on the 10th were near the star *Alpha*.

The number of stars seen this year on August 10th was about eighty, the sky being clear for an hour, from shortly before 10 o'clock to near 11 o'clock. Fifty-five of these had their paths and other features recorded here; out of this number the following are those noticed proceeding from the direction of these two points of divergence:—

From Cygnus	23
From Cassiopeia	26
Discordant	6

55

In 1848.

From Cygnus	5
From Cassiopeia	8

13

The two following letters to Prof. Powell will further illustrate this point.

“My dear Sir,—The opinion that falling stars diverge from a given point at two periods of the year, viz. August the 10th and November the 11th, is generally believed, but I have seen no hint that they do so at other times; that they nearly all do I feel perfectly persuaded. From numerous observations on the 20th and 21st of this month, I find they diverge from about the centre of the constellation Cygnus; last night (July 23rd), which was particularly rich in falling stars, gave a position slightly different, viz. ζ Cygni for the mean point from which they diverged; if this point was more attended to, in all probability we should soon have sufficient data to enable us to give at all events a rough element. From a few observations during June, this point would seem to be in Draco, in beginning of May in Bootis, and in April in Canes Venatici. The meteors were few in number in April, May and June, but are now each night becoming much more numerous.

“The tail, as it is called, of meteors is apparently of two kinds, the one a continuous streak of light, and the other individual sparks; this does not seem to be owing to the speed with which they move, for I have frequently seen each appearance, whether the meteor was moving rapidly or slowly.

“Believe me yours very truly,

“Highfield House Observatory, near Nottingham,
July 24, 1849.”

“EDWARD JOSEPH LOWE.”

“July 28, 1849.

“My dear Sir,—Out of the nine observations on the 26th of July, six gave a point of divergence slightly below ρ Cygni. Some of the observations last night gave a position rather lower in the Swan. It is pretty evident there is a point of divergence, and that this point is now situated in the Swan, for each night produces more examples of meteors coming from that direction.

“Believe me yours very truly,

“EDWARD JOSEPH LOWE.”

In 1842, Prof. Phillips noticed many meteors came from the direction of Cassiopeia, and in 1848 the Rev. C. Marriott at Bradfield again noticed this feature in one instance.

It seems quite evident that the greater portion of these bodies move in lines parallel to each other; for as proof that the point of divergence is merely owing to perspective, the greater number of stars to the S. and S.E. of us move towards S. and S.W., whilst those to W. and N.W. move towards W. and W.S.W. This was very evident on the 10th of August this year, for a great number which occurred in Pegasus all moved to S. and S.W., whereas

those in Ursa Major, Ursa Minor and Draco descended towards W. and W.S.W.

(4.) Interesting features in Meteors of August 10th, 1849.

In fifty-five meteors recorded on this night, in eleven cases a second falling star moved almost immediately afterwards in the same or nearly similar track to that which had just gone before; these occurred at

^h	^m	"			^h	^m
9	56	0	followed by another in same track in		2	0
9	59	0	"	"	"	1 0
10	3	0	"	"	"	1 0
10	5	30	"	"	"	0 30
10	7	0	"	"	"	0 30
10	8	0	"	"	"	0 30
10	11	0	"	"	"	0 15
10	13	0	"	"	"	1 0
10	17	0	"	two others	"	{ 0 15*
						{ 0 30
10	30	0	"	another	"	0 3
10	38	0	"	"	"	0 30

The meteors on the 10th mostly moved exceedingly rapid; 48 are entered as moving quickly and only 6 as slow. They were generally accompanied by continuous streaks of light, which they left behind them for one or more seconds; that which occurred at 10^h 5^m 30" left a ray of light visible for 31" after the head of the meteor had vanished, which was 7° in length. 24 are entered as having tails, and 4 without tails.

The number seen each five minutes during the hour they were visible, was

^h	^m	^h	^m	Falling stars.
from 9	56	to 10	0	4
	10		5	3
			5	9
			10	4
			15	6
			20	6
			25	4
			30	4
			35	6
			40	3
			45	4
			50	2

The distribution of colours amongst the meteors, was—

Colour.	No. of Meteors.
Yellow	14
Red	7
Blue	5
Colourless	2

The apparent size, as compared with other objects, was as follows:—

	No. of Meteors.
Rather larger than Venus when nearest ⊕....	1
Size of Venus	5
" Jupiter	6
" 1st mag. star	8
" 2nd mag. star	3
" 3rd mag. star and smaller	32

* At this time three moved in the same track.

In the 55 falling stars 8 became first visible in Pegasus, 8 in Cygnus, 6 in Andromeda, 5 in Ursa Minor, 4 in Delphinus, 4 in Cassiopeia, 3 in Antinous and Ursa Major, 2 in Aquila, Aquarius, Lyra, Corona Borealis and Cepheus, and 1 in Pisces, Bootes, Perseus and Camelopardalis. Of these 8 became extinguished in Draco and 8 in Pegasus, 6 in Antinous, 4 in Pisces, 3 in Cygnus, Aquarius, Lyra and Bootes, 2 in Sagitta, Cassiopeia, Ursa Major and Perseus, and 1 in Andromeda, Delphinus, Ursa Minor, Coma, Berenices, Serpens, Capricornus, Hercules, Camelopardalis and Triangulum.

It is pretty evident that the meteors were nearer us on the 10th of August very considerably than on the 16th, as on the latter day, although a few tolerably sized ones were seen, yet the great majority were meteors very brilliant for their size, which was smaller than the smallest stars that could be discerned by the unassisted eye.

(5.) It is a curious fact, that when a falling star is seen to follow another in the same track, it invariably moves at an equal speed with the one which had gone before, *i.e.* if the first moved rapidly the second would do so also, and if slowly the second would move slowly. The second star I have never as yet seen larger than the first; and generally there has been a considerable difference in apparent size, from the circumstance of the follower of a falling star in the same track partaking of the speed of that which has gone before, and that generally the respective bulks are very different; it might be supposed that the smaller one was an attendant or satellite of the larger one; if this be the case, the meteor that fell at 10^h 17^m was accompanied by two satellites; this strengthens the opinion very much of their being material bodies.

On the other hand, if we consider them as shining by reflected light, it is difficult to account for the *luminous streak* which is often left in the sky after the head of a meteor has itself vanished, and also why a meteor *having a continuous ray of light*, if it cross an auroral arch or beam, instantly brightens, a circumstance exceedingly curious and at the same time very apparent: the phenomenon has been noticed here four times, *viz.* December 3, 1845, September 10, 1846, June 21, 1847, February 20, 1848.

As there are several difficulties attending this phenomenon if we account for them all with one theory or consider them all to be similar in formation, I have ventured to suggest three classes:—

- 1st. Those with luminous streaks.
- 2nd. Those with separate stars, and those without any appendage.
- 3rd. Those large bodies with well-defined discs.

The 1st class may shine by *inherent light* or be surrounded by a *luminous atmosphere*; the 2nd class by reflected light, as described by Sir John Lubbock; and the 3rd class may be purely atmospherical; as this kind nearly always move in paths discordant to the direction of the other meteors, they are not always spherical, and sometimes change their form: I have seen them alter their colour from blue to red, and in one instance saw a meteor of a blue colour give out orange-red sparks. Mr. Hind tells me he saw a green meteor turn to a crimson colour.

I have made numerous inquiries, but could never find any one, excepting Mr. Hind, who had seen meteors move slowly across the field of a large telescope; he describes them as appearing better defined than stars, which they resemble, but the time of visibility was too short to allow of a planetary disc to be discovered; the fragments or streamers appeared like phosphoric lights.

No. 18.—*Details of Observations of Meteors.* By W. R. BIRT.

Projection in the plane of the horizon of fourteen shooting stars, observed at Highfield House, near Nottingham, by E. J. Lowe, Esq., on the evening of the 10th of August 1849, between 9^h 56^m P.M. and 10^h 16^m P.M.

The circle bounding the projection represents the horizon, and its centre the zenith.

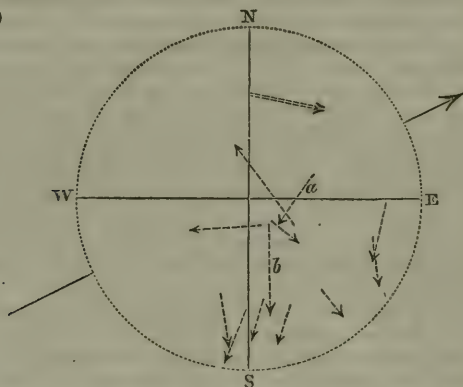
The line N—S the meridian arc.

The line W—E the *prime* vertical.

The arrow-head indicates the direction of the earth's orbital motion at the time of observation, the portion of the horizon N, W being directed towards the sun.

The shooting stars marked *a* and *b* are considered to be identical with two observed at London.

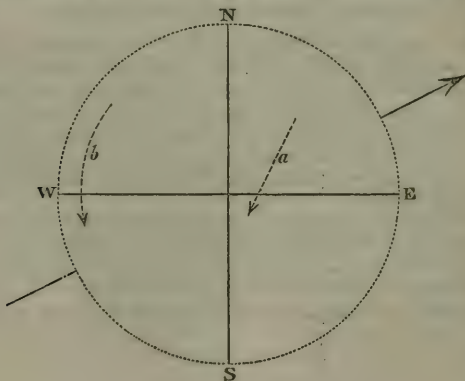
(1.)



Projection in the plane of the horizon of two shooting stars observed at London by W. R. Birt, near 10 P.M. of the evening of the 10th of August 1849.

The letters *a* and *b* refer to those shooting stars observed at Nottingham, which are considered to be identical with them.

(2.)



(3.) Observations on luminous meteors, August 10, 1849.

a No. 1. Between 9^h 30^m P.M. and 9^h 33^m P.M., a rather large luminous meteor shot from the Via Lactea, nearly midway between Cygnus and Cassiopeia; it was nearer Cassiopeia than Cygnus; its path was apparently *straight*, across Lacerta, and the point of disappearance between Cygnus and Pegasus considerably below the former and much nearer the latter. It scarcely passed, if at all, the line joining α Cygni and β Pegasi. It left a train of reddish scintillations, which were more conspicuous about the *middle* of its course, dying away at each extremity.

a No. 2. A small meteor, between one and two degrees east of Polaris, passing downwards.

a No. 3. A luminous meteor passing between α and β Pegasi, and cutting the line joining them very obliquely; its direction appeared to be parallel to the meridian towards the south; it was small and of short duration, but rather larger than No. 2.

a No. 4. A small luminous meteor, near the head of Capricornus, passing south and west.

a No. 5.

a No. 6. At about a minute or two before 10^h P.M., a rather large luminous meteor shot from above and to the north of Cassiopeia and disappeared just to the north of Cygnus, the points of appearance and disappearance being within the boundaries of each constellation; its apparent path, which was *straight*, appears to have crossed the head of Cepheus. It was attended by a train of reddish scintillations, more conspicuous in the *middle*, and dying away at each extremity. This meteor was extremely similar to *a* No. 1 in every respect, save one, viz. direction. The directions of these meteors did not appear to be parallel, but such as to give the idea of divergence; the line of direction of *a* No. 1 produced *backwards*, intersecting that of *a* No. 6 about the point of its commencement.

The above meteors were the only ones seen between 9^h 30^m P.M. and 10^h. They were all of a blue colour.

b No. 1 (?). At a very few minutes after 10^h P.M., a very large and bright meteor shot from beneath the tail of Ursa Major (the constellation at the time being covered with a cloud), most probably in the neighbourhood of Cor. Caroli; it crossed about the middle of Bootes, ϵ Bootes (?), and vanished to the south of Corona Borealis. I much regret I did not obtain a full view of this fine meteor, which was by far the largest hitherto seen, and I should conceive exhibited the longest path, being engaged at the moment in contemplating Cassiopeia. The light attracted my attention, and on turning I just caught a sufficient glimpse of the meteor to assure me of its magnitude and direction. I had a full view of the train of reddish scintillations which it left behind; this train perfectly agreed with the two I had previously witnessed; its direction appeared to be very nearly if not quite parallel to the horizon; it indicated that the path of the meteor was *straight*.

b No. 2 (?). A small but bright meteor passed *directly over* Polaris, bisecting the star; its course appeared to be in the meridian towards the horizon.

N.B. I am not quite certain which of these meteors occurred earliest, but I strongly suspect that *b* No. 1 was first.

b No. 3.

b No. 4. A very splendid globular meteor, about the size of Jupiter at opposition, of a whitish colour, very slightly tinged with red, passed with a comparatively slow motion immediately over γ Pegasi (the star was bisected). Its path, which was slightly curved, was nearly parallel to the horizon, and the meteor increased in brilliancy as it proceeded until its disappearance,

the point of which could not have been far removed from the line joining α and β Pegasi, and produced: γ Pegasi was about midway of its apparent path. The path suggested the idea of that of a projectile, the meteor sensibly bending to the earth just before the disappearance: there was no train, and the meteor was exceedingly unlike any of the preceding.

b No. 5. Within a very short interval, I should say less than a minute, another meteor, of precisely the same size and exhibiting precisely the same characters in every respect, not one excepted, appeared just beyond the point of disappearance of *b* No. 4. Its path appeared to be a prolongation of that of *b* No. 4, and it disappeared in exactly the same manner, slightly bending to the earth, or rather horizon, not far from β Aquarii.

Assuming for the moment, which is not altogether improbable, that the two were only one meteor, which by some means had been extinguished for a short time during its progress, its visible path in the heavens at London would at least be from 15° north Dec. 0° Rt. Asc. to 6° south Dec. 320° Rt. Asc. retrograde, and it crossed the equinoctial about 332° Rt. Asc.

b Nos. 1 to 5 occurred by estimation between 10^h P.M. and $10^h 15^m$ P.M., certainly not later than $10^h 20^m$ P.M. The appearance of *b* Nos. 4 and 5 most probably occurred at $10^h 15^m$ P.M.

Remarks on the above Meteors.

Of the above meteors five claim particular attention, viz. *a* Nos. 1 and 6 and *b* Nos. 1 (?), 4 and 5, *a* Nos. 1 and 6 occurring previous to 10^h P.M., and *b* Nos. 1 (?), 4 and 5 after 10^h P.M. Each of these meteors are very readily identifiable. After *a* No. 1 had appeared, a considerable time elapsed before *a* No. 2 was seen in the neighbourhood of Polaris, and the *directions* of these meteors were very different and nearly opposite. Nearly half an hour elapsed before *a* No. 6 became visible, its direction and that of *a* No. 1, as before remarked, indicating a point of divergence just north of Cassiopeia. With the exception of the meteors in the neighbourhood of Polaris and that between α and β Pegasi, those seen in the eastern hemisphere, viz. *a* Nos. 1, 4 and 6, and *b* Nos. 4 and 5, were directed more or less *towards the meridian*. *b* No. 1 (?) was the only meteor seen westward of the meridian; and it is worthy of remark, that while the direction of its motion was *towards the meridian*, it was in the opposite direction to those in the eastern hemisphere, and this appears to indicate a point of *convergence* in the south, as well as a point of *divergence* in the north. These phenomena may greatly assist in determining the position in space of these bodies. It is clear that at the time of observation, the earth was moving towards a given point in the heavens. The general direction of the meteors in the eastern hemisphere was retrograde, while that in Bootes was direct. Assuming for a moment that between $9^h 30^m$ and $10^h 30^m$ the earth passed very near a small cluster of meteoric bodies, which was moving in a contrary direction, the majority being *south* of the earth's path, and one only *north*, the phenomena would be just as witnessed. All the *southern* meteors would have a retrograde motion, while that of the *northern* would still coincide in the general direction of motion; but instead of its being *retrograde* it would be *direct*, just as a traveller on a railway sees the objects apparently rushing past him on each side, their apparent motions being identically the same; yet when these motions are referred to the circle of which he is the centre, it is evident those on his left-hand must have an opposite expression to those on his right.

Upon a comparison of the paths of *a* Nos. 1 and 6 and *b* No. 1, which appear, with the exception probably of the globular meteors *b* Nos. 4 and 5, to have been nearest the earth, we may be better able to judge of their

relative positions, especially with respect to the earth. Taking Lacerta as the middle point of the path of *a* No. 1, the head of Cepheus as that of *a* No. 6 and ϵ Bootes as that of *b* No. 1, it is very evident that the earth, or at least its centre, must have passed considerably to the *south* of the plane passing through the centres of *a* No. 6 and *b* No. 1, and as *b* No. 1 appeared but a few minutes after *a* No. 6, the distance between them would be considerably less than that between *a* No. 1 and either of the others. If, as has been suggested, the direction of the earth's motion was such as to leave the meteor *b* No. 1 on the north and *a* No. 6 on the south, both would be sufficiently identifiable at any part of the earth's surface from which they might be visible:—1st, from the priority of the southern meteor; and 2nd, from the apparent opposition of their motions; and should observations have been made from which the altitudes of each above the earth's surface may be deduced, it would not be very difficult to determine approximately and within certain limits their distance from each other, due allowance being made for the earth's motion between the instants of apparition. In connexion with the view here taken of the relative positions of these three bodies, the *straightness* of their paths strongly indicates the passage of the earth past them. Upon M. Quetelet's determination of the *mean* altitude of these bodies being sixteen or twenty leagues, it would appear that when the nearest point of the earth's surface approaches a meteoric body at or within this distance, the phænomena witnessed would be produced: the body would pass through a segment of the earth's atmosphere, the path most probably differing but little from a straight line; upon entering the earth's atmosphere combustion may take place, as suggested by Prof. Powell, and this may give rise to the *reddish scintillations* so apparent in the three bodies observed; these scintillations presented phænomena perfectly in accordance with this notion, being most intense in the middle or *deepest* part of the earth's atmosphere, and gradually dying off at each extremity.

The meteors *b* Nos. 4 and 5 appeared to be essentially different from the three we have just noticed; the well-defined globular appearance they presented, the comparative *slowness* of their motion, the slight curvature of their paths, and their decided increase of brilliancy just previous to their extinction, place them altogether in a different category, and would lead one to expect that at more southern stations they appeared both larger and more brilliant. It would be interesting to obtain observations of these meteors (which certainly were unmistakeable in their character) from places at which they were vertical. At present however we must be content with knowing that of the group of meteors observed they were probably the most southern, the plane of their motion being less inclined to the ecliptic than to the equinoctial.

Notice of Nebulæ lately observed in the Six-feet Reflector. By the
 EARL OF ROSSE, Pres. R.S. *Communicated by the Rev. Dr.*
 Robinson, Pres. B.A., *and ordered to be printed entire among the*
Reports.

At the Meeting of the British Association at York in 1844, it was announced that a reflecting telescope of six-feet aperture, which had been about two years in progress, was nearly completed, and some slight account was at the same time given of the means which had been taken to render the instrument convenient and effective. A short notice of the principal results

which have since been obtained may perhaps not be uninteresting to the present meeting.

About the beginning of February 1845, the instrument was so far finished as to be usable; and in the first instance it was directed to some of the brighter nebulae in Herschel's Catalogue. Many of them were immediately resolved, and very frequently the aspect and form of well-known nebulae were completely changed, fainter details not previously seen being brought out by the great light and magnifying power of the telescope. Before the end of April the wonderful spiral arrangement in 51 Messier was discovered. The speculum, though there was a slight defect of figure, was in fine working order, and defined with great sharpness when the air was steady.

At the approach of the short nights, when the season for observing the nebulae was nearly over, the instrument was dismounted, as it was desirable to take the earliest opportunity of completing certain portions of the mechanism which had been put together in a temporary way in a rough state, and it was not till the close of the year that it was again in working order.

During the year 1846 the examination of the nebulae in Herschel's Catalogue was continued; many sketches were made, and another spiral nebula was discovered, 99 Messier. The moon was observed occasionally, and the superiority of the instrument with six-feet aperture over that of three under equal magnifying powers, in bringing out minute details, was very remarkable; so great is the effect of light even when we have to deal with an object so bright as the moon with an aperture of three feet.

As yet, however, but little time has been devoted to an examination of the moon: the moonlight nights have usually been taken advantage of for experiments on the polishing and figuring of the mirrors; and the information which has been obtained relates principally to matters of detail, from which it would be premature to attempt to deduce general conclusions suitable to the present notice.

In the succeeding year, 1847, there was but little done; unprovided at that time with an assistant capable of making trustworthy use of the pencil and micrometer, and being almost wholly occupied with the duties incidental to a year of famine, it was impossible to do more than re-examine a few of the objects of the previous year.

From the beginning however of the year 1848 till the present time, the instrument has been constantly employed whenever the season and weather permitted it, and the following are some of the results:—H. 604 was found in some degree to resemble the great spiral nebula 51 Messier, but it is a much fainter object, and appears to be made up of elliptic streaks disposed rather irregularly with a tendency to spirality, but without that distinct symmetrical spiral arrangement which is so marked a feature of 51 Messier. If 51 Messier were seen somewhat obliquely, and were considerably fainter, it would probably very closely resemble it.

H. 854, M. 65, has an arrangement of very elliptic annuli, and is apparently a system of the same class seen very obliquely.

M. 97, H. 838, is a very extraordinary object; with a dark hollow centre somewhat in the shape of a figure of eight, easily seen, and with a disc irregularly shaded, but showing in the shading a decided tendency to spirality when seen under favourable circumstances: two stars are placed in a remarkable manner in the central opening. We may conceive it to be a spiral system greatly compressed; the edges are filamentous: H 2205 has a faint but large spiral appendage, to which the ray as figured by Herschel is in some measure a tangent. Several other nebulae are recorded in our note-books as belonging to the class of spirals. The well-known planetary nebula in Aqua-

rius, H. 2098, which in former years had been often examined with a telescope of three-feet aperture, and with no other result than that it exhibited a filamentous edge, when seen with the great instrument was found to have two ansæ like Saturn. Many have since seen it, and the resemblance to Saturn out of focus has usually suggested itself. It is probably a globular system surrounded by a ring seen edgeways; while H. 450, which turns out to have a bright centre surrounded by a comparatively dark ring, and that again by a bright ring, though a much fainter object, is not improbably a system of the same character seen directly.

H. 84 and 86 is a remarkable group of nebulae; it consists of eight, two of them pretty bright. Such groups are not uncommon, but in this instance there are I believe more nebulae in a given space than in any other group we have noticed; it was observed by Mr. Stoney. The nebulae were not connected by any perceptible nebulosity, but there are cases where a nebulous connection was distinctly traced; several minute nebulae or nebulous knots hanging together as it were by a very faint but unmistakeable nebulosity. The nebulae of Andromeda and Orion have of course been observed. As to Andromeda, there seems to be little doubt that the companion is resolvable, and the nucleus of the great nebula has that granular appearance which indicates resolvability: it has however not been seen as yet under very favourable circumstances, and we have not commenced a sketch of it. The nucleus was examined on three occasions, and the abrupt edge of the following streak in Mr. Bond's drawing was traced to its visible limits; but unfortunately we did not receive the drawing till the nebula was out of reach, otherwise of course more attention would have been directed to it. Subsequent to the receipt of the drawing, the nebula was seen by Mr. Stoney in my absence with the instrument of three-feet aperture, but at a distance from the meridian: the appearance was very much as in Mr. Bond's drawing, except that the contrast between the preceding portion bounded by the edge of the following streak, and the following portion of the nebula was much greater.

The question however of most interest is, what do these streaks indicate? With the great instrument, dark streaks have been observed in many of the nebulae, sometimes almost straight, as in Andromeda; for instance, H. 887, H. 1909, H. 1041, H. 1149, are cases in point, the streaks being nearly straight. H. 1357, to which Mr. Bond refers, is, if possible, a still stronger case than it appears to be by Herschel's drawing, as I find a sketch in our journal showing that the appendage is part of the nebula, the nebulosity extending and encasing both extremities of the opening just as in Andromeda. We have also found a variety of examples of curved streaks; for instance, H. 264, H. 491, H. 406, H. 731, H. 854, H. 875, H. 1225, and others.

Also H. 1486, H. 464, H. 2241, besides the well-known annular nebula, and the little annular nebula sketched by Herschel, are some of the examples of nebulae with comparatively dark centres; the darkness being apparently of the same quality as the dark streaks, but of a different shape.

With these facts therefore I think it not improbable that the dark lines noticed by Mr. Bond in the nebula of Andromeda, and which with sufficient power are perceptible in so many other nebulae, sometimes nearly straight, sometimes variously curved, as also the dark spaces, are all indications of systematic arrangement. When we see a dark space in the centre of a planetary nebula, it is impossible to resist the impression that we are looking at an annular system bound together by some mysterious dynamical law. If we see a bright centre, as in H. 450, surrounded by a dark annulus, and that again by a bright annulus, we have a system of another kind; and in the spirals, of which 51 Messier is the most remarkable example we yet have found,

we have a regularity of arrangement equally accordant with our preconceived notions of the order which should subsist in a regular independent system.

The very elongated elliptic annular nebulæ, where the minor axis is sometimes almost evanescent, show us pretty clearly the nature of the slight, long, dark and nearly straight streak in some cases found parallel to the axis of a long ray. A little consideration of the appearances which annular and spiral systems must present when viewed in different positions, in some instances affords a pretty satisfactory explanation of the confused streakiness we have observed in several of the nebulæ.

This, however unsatisfactory it may appear, is the best explanation our working journal-books at present afford of the streaks observed by Mr. Bond in the nebula of Andromeda.

Mr. Bond's paper has excited so much interest, and I have been so often questioned relative to it, that I have prematurely, in anticipation of more numerous sketches and measurements, which will probably throw additional light on the subject, ventured to lay before the Association the very little, which is at present known to us.

It was in the spring of 1846 that we first perceived the brighter portions of the nebula of Orion in the neighbourhood of the trapezium breaking up into minute stars. Whenever the sixth star was nicely separated, this appearance was clearly perceptible. We had repeatedly examined Orion with the telescope of three-feet aperture, without a suspicion of its being resolvable; however, its resolvable character once known, we were enabled with it on very fine nights to see some of the stars. With the six-feet telescope, the space within the trapezium is still dark, just as Herschel describes it, and I feel convinced there is no optical illusion.

Last season my attention was directed by Mr. Stoney to ϵ Orionis, which is on the edge of a dark spot; the dark spot includes the nearer companion, and is about 12" diameter; we have not yet had an opportunity of examining it with the great instrument.

A few copies from our collection of sketches accompany this notice: they have been made within the last day or two by a drawing-master in the neighbourhood. He has transposed white for black, and enlarged the scale to make them more suitable for exhibition in the Section.

In sketching, we employ solely the black-lead pencil, black representing light, and the eye by habit makes the transposition without effort.

The copies are not quite accurate, but they are sufficiently exact for the purpose.

On the Influence of Carbonic Acid Gas on the health of Plants, especially of those allied to the Fossil Remains found in the Coal Formation. By Professor CHARLES G. B. DAUBENY, M.D., F.R.S. &c.

"AT the Meeting of the British Association for the Advancement of Science held at Oxford in 1847, it was resolved, that a Committee, consisting of Sir H. T. De la Beche, Sir W. J. Hooker, Dr. Daubeny, Dr. J. D. Hooker, Mr. A. Henfrey, and Mr. R. Hunt, be requested to investigate the influence of carbonic acid on the growth of plants allied to those found in the coal formation."

This investigation was accordingly entered upon by myself in the spring of 1848, by means of an apparatus consisting of two jars of corresponding size, each containing about 2800 cubic inches of air, the edges of which

rested upon a smooth slate table, having two circular holes perforated in it, into each of which a pan or pot containing the plants to be experimented upon was inserted.

By the aid of this apparatus I carried on a series of experiments both on flowering plants and on ferns, from which I inferred that the one as well as the other would continue for a fortnight at least unaffected by a dose of carbonic acid, bearing a proportion to the whole volume of air equal to from 5 to 10 per cent., but that 20 per cent. would prove injurious to the one, as well as to the other, in the course of two or three days. These results were however not offered to the Association at Swansea with any confidence, because the apparatus contrived for the purpose of carrying them on turned out to be defective, the difficulty of cementing the vessels containing the plants to the slate table, so as to render the apparatus impervious to air, being such, that a large supply of gas was each day found requisite, in order to keep up the per-centage to the intended amount. Hence it was probable that during a portion of the time the real quantity of carbonic acid in the jar might have fallen very short of that with which it was proposed to operate.

I therefore renewed the experiments in the spring and summer of the present year 1849, in two ways, either of which had been ascertained by previous trials to preclude in a great degree the danger of leakage, and thus to render the amount of carbonic acid present whilst the experiment was being carried on, tolerably constant.

The first was that of allowing the jars, the edges of which had been well ground, to rest upon the surface of a solid and smooth slate table, greased along the line of its contact with the glass; the other to make them dip into shallow iron dishes with double rims, containing water to the depth of an inch, so that the air of the jar might be cut off from the external atmosphere. In neither of these cases was there a sufficient loss of gas to interfere with the results; in the former, the transmission of air between the smooth surfaces of the slate table and the jar being inconsiderable, and in the latter, the quantity of gas carried off by solution in the water being much reduced, when the latter was covered with a thin pellicle of oil. Whatever indeed might be the loss in either case sustained, I took care to supply it, by introducing the requisite quantity once every twenty-four hours into the jars which contained the plants.

I am therefore now able to offer to the Association, with rather greater confidence than before, the following results, as confirmatory of those which were stated verbally in my Report, but which, for the reasons already assigned, were not published in the Transactions of the Association for last year.

May 14th.—In the first experiment, five healthy ferns, named *Nephrodium molle*, *Adiantum cuneatum*, *Gymnogramma chrysophylla*, and two species of *Pteris*, viz. *longifolia* and *serrulata*, were introduced into jar 1 standing in water, and a quantity of carbonic acid gas was admitted, which equaled 5 per cent. of the whole amount of air present in the jar. No perceptible change occurring, the quantity was increased on the 17th to 10 per cent., and this amount was maintained, as nearly as possible to the same point, by occasional additions of the gas, till May 27th.

At the expiration of ten days there was no perceptible difference in the appearance of the ferns, either with reference to their preceding condition, or by comparison with that of five similar ferns, which had been kept for the same time under the corresponding glass, without any admixture of carbonic acid gas. The experiment was then continued till June 21st, so that the plants were exposed to the influence of carbonic acid gas in all for a period of thirty-

three days, besides being subjected for seven days to about 5 per cent. of the same. At the end of this time only two of the ferns appeared at all damaged, namely *Pteris longifolia* and *Nephrodium molle*, the fronds of both which were rather discoloured, those of the other three species remaining as before.

The same description of experiment was made upon a species of *Pelargonium*, which after having been during twenty-seven days exposed to the action of 10 per cent. of carbonic acid contained in the air of a large jar, appeared in exactly the same condition as a corresponding one placed under glass in a vessel free from any abnormal mixture of that ingredient.

From these, and from the experiments of the preceding year, it might be inferred that plants in general are tolerant of a much larger volume of carbonic acid gas than exists in the atmosphere at present; but it did not therefore follow, that the amount of carbonic acid decomposed, and of oxygen exhaled, would bear any proportion to the quantity with which their leaves were brought into contact.

From several trials indeed which I made as to the per-centage of oxygen present in the jar at different stages of the experiment, I was led to infer that the amount of the latter was not increased in the degree which might have been expected; but, as a more easy way of determining the same point, I introduced a certain number of fresh leaves of an *Helianthus*, in each case exposing exactly the same amount of surface, into jars filled with water containing different proportions of carbonic acid gas. In No. 1, for instance, the proportion of gas to water was only as 1 to 12; in No. 2 as 1 to 6; and in No. 3 as 1 to 3. Now it was found, that, instead of the oxygen disengaged by the leaves keeping pace with the supply of carbonic acid, only 0·7 of a cubic inch was given off from No. 3, whilst No. 2 had disengaged 4 cubic inches, and No. 1 3·3 cubic inches; and in another experiment only 0·1 was emitted by No. 1; 4·5 by No. 2; and 2·0 by No. 3, the other circumstances, as to time, exposure to light, &c., being in all cases the same. If therefore the disengagement of oxygen from leaves be, as is generally admitted, the result of their vital action upon the carbonic acid in contact, under the stimulus of light, it would follow, that where the carbonic acid exceeds a certain amount, that action is in a great degree suspended.

There is however an experiment of Count Rumford's, originally reported in the *Philosophical Transactions* for 1786, and alluded to by one of my coadjutors in these investigations, I mean Mr. Hunt, in his late work entitled 'The Poetry of Science,' which would seem to imply that the decomposition of carbonic acid by plants was not a vital phenomenon, and consequently could not be influenced by any such circumstance as the application of a superabundant portion of this gas to the surfaces of their leaves.

Count Rumford states, that the property of causing water to emit oxygen in the sun, is possessed, not only by living plants, but likewise by threads of silk, by wool, and even by spun glass; in which case the decomposition of carbonic acid would seem to be simply the effect of light, the plant merely serving, by the surfaces it exposes to the water, to disengage from it, more rapidly than would otherwise happen, that oxygen which had been obtained without its direct agency.

On repeating this experiment, however, I found, as might have been anticipated, that at first no such effect took place when wool, cotton, silk, or spun glass were introduced into the water, but that after some days it occurred abundantly in every one of these cases—the disengagement of the gas however being always coincident with the appearance in the liquid of green *confervæ*, to the action of which doubtless this decomposition of carbonic acid was to be attributed.

Accordingly the process went on, whether fibrous substances were placed in the water or not, although in the latter case somewhat less rapidly, the presence of such bodies serving to disentangle the particles of gas from their adhesion to the water more easily than would happen otherwise.

There cannot therefore be a doubt, that the common opinion, which regards the emission of oxygen from the surfaces of leaves, whether placed in water or in air, as a *vital* phenomenon, is the correct one, and hence it is quite consistent with analogy, that, as we have already seen, some one proportion of carbonic acid in the air should be more favourable to the exercise of this function, than any other one more considerable in amount would prove.

I was therefore encouraged to proceed in my inquiry as to the quantity of carbonic acid contained in air, which was decidedly prejudicial to the health of ferns.

With that view specimens of the same five species as before were selected for experiment, and these were placed under the jar which contained about 2800 cubic inches of air cut off from the external atmosphere by water. To this air 1 per cent. of carbonic acid was at first added, and a daily increase to the same amount in the quantity present was kept up, until the proportion reached 20 per cent. This same quantity was then maintained in the jar for twenty days, by successive additions to compensate for the ascertained amount of leakage, now found to be inconsiderable, and the appearance of the plants was from time to time examined and noted.

It was not till the 13th day that any sensible alteration for the worse was perceptible, when we observed, that in *Pteris longifolia* the fronds had become very brown; in *Nephrodium molle* and in *Gymnogramma chrysophylla* two or three of the lower fronds showed signs of yellowness; that those of the *Adiantum* looked in general very sickly, but that *Pteris serrulata* did not appear injured. The experiment was however continued seventeen days longer, when it was found, for the first time, that the amount of carbonic acid present in the jar, as ascertained in the usual way by potass, exceeded what had been added; proving more decisively than before, that decay had commenced. The plants were accordingly taken out, and the following notes respecting their condition were entered in the Minute-Book.

Pteris longifolia.—All the old fronds are now dead, but the vitality of the rhizoma is not destroyed, for young fronds are putting out, and appear at present to be healthy.

Pteris serrulata even now appears but slightly damaged, its fronds being only more yellow than is natural.

Nephrodium molle seems in the same condition nearly as *Pteris longifolia*.

Gymnogramma chrysophylla.—Its old fronds slightly damaged and yellow, but young ones are putting out.

Adiantum cuneatum.—All the fronds have died down.

Thus it appears that this large amount of carbonic acid, even if gradually added, would in time prove fatal to plants of the above description, although operating upon them with various degrees of intensity, and apparently not exerting any specific influence upon the stem and roots.

That the effect however was attributable; not to the diminution in the proportion of oxygen consequent upon the addition of so large an amount of carbonic acid, but to something positively deleterious in the latter gas itself, was inferred, by exposing the plants to air impregnated with 20 per cent. of hydrogen, which in the course of ten days appeared to exert no sensible influence upon their health.

There did not appear to be any *very material* difference in the action of

carbonic acid upon plants, whether it were suddenly or gradually introduced; for when I exposed the same ferns to air into which 20 per cent. of carbonic acid had been added all at once, it was not till the 9th day that any change in their appearance was perceptible, and then only in three of the specimens; *Pteris serrulata* and *Adiantum cuneatum* being scarcely, if at all affected.

However, on the 16th day the influence of the gas was manifest upon all except *Pteris serrulata*; the per-centage of carbonic acid was found to exceed that which had been added from without, and the condition of the ferns generally was rather more unhealthy and faded than it had been in the foregoing experiment, where the gas had been added in successive doses*.

So much for this part of the investigation, which seems to be in a manner prefatory to the one which may be regarded as the more immediate object aimed at by the Association in suggesting these researches, that being, whether a larger amount of carbonic acid than is present in our atmosphere would increase the vigour, and stimulate the growth, of the tribes of plants which are most connected with the fossil remains found in the coal formation.

With reference to this latter question, I am not so far advanced towards its determination as might have been desired.

During the last five weeks Ferns and Lycopodiums have been living in an atmosphere containing constantly 5 per cent. of carbonic acid, whilst corresponding specimens have been placed under similar circumstances, except that the abnormal amount of carbonic acid above stated was absent from the air of the jar. In both instances the Lycopodiums continue up to this time in perfect health, but it must be confessed that the *Adiantum cuneatum* and *flagelliforme* which have been subjected to carbonic acid appear less thriving than the corresponding plants not so treated.

It must be remarked moreover, that the per-centage of gas within the former jar has been increased to $5\frac{1}{2}$ per cent., the additional $\frac{1}{2}$ per cent. being attributable to the diseased state of some of the fronds.

The experiment however shall be continued for a longer period until more decisive results have been arrived at.

But supposing it to be ascertained that ferns will exist in air containing 5 per cent. of carbonic acid, it still remained a question, whether the animals that lived at the same period could have resisted the poisonous influence of so large a proportion of this gas.

In the coal formation, properly so called, Mollusca and Fish appear to be the animal remains principally detected, and the difference between the structure of existing species, and of those which were in being at so remote a period as the one alluded to, may be urged, as an objection to the idea of extending to the latter any inferences that might be deduced from experiments instituted upon the former.

Nevertheless as in so fundamental a function as that of respiration, a similar law pervades all the individuals belonging to the same great natural group at the present time, as for instance, what is true in this respect concerning the lowest in the scale of Mammalia, holds good likewise with certain modifications with regard to the highest, it may not be illogical to presume, that the difference as to time would not create any radical change in the relations of a particular class of animals to carbonic acid, and in their susceptibility to its influence.

* I do not find that ferns suffer from confinement in large jars; and at all events, as the circumstances were precisely the same in the two cases, with the exception of the presence or absence of this excess of carbonic acid, the difference in the appearance of the specimens seems clearly referable to the latter cause alone.

With reference to the proportion of carbonic acid which water would abstract from air containing diffused through it so large an amount as 5 per cent. of this gas, the principles upon which such a problem may be determined have been long ago clearly laid down by Dr. Dalton.

As neither carbonic acid, oxygen, or nitrogen are retained in water by virtue of any chemical affinity, but simply in the ratio of their respective elasticities, it follows that the quantity of these gases present in it will be regulated by the amount of each existing at the time in the superincumbent air.

We know by experiment, that water would retain nearly about its own volume of carbonic acid; 0.65 of its volume of oxygen; and 0.42 of nitrogen; under the pressure of an atmosphere consisting wholly of the gas so retained.

If therefore we suppose the atmosphere in former times to have consisted of carbonic acid 5 per cent. and of common air, maintaining its present constitution, 95, that is, of—

Nitrogen	76
Oxygen	19
Carbonic acid	5

the quantity of each gas retained by a volume of water under such circumstances would be as follows:—

Nitrogen	·03192
Oxygen.....	·01235
Carbonic acid	·05300

·09727*

Water therefore under an atmosphere of this constitution would still contain nearly as much oxygen as it does at present, and not more than .05, or $\frac{1}{20}$, of its volume of carbonic acid, so that the condition of the gas expelled from the water would be such, as to consist in 100 parts of—

Carbonic acid	54.5
Nitrogen	32.9
Oxygen.....	12.6

100.0

Now I am enabled to prove, that a much larger proportion of carbonic acid than that supposed may exist in water without affecting the health of fish at the present time. On one occasion indeed I agitated some river water in a closed vessel with a mixture of common air and carbonic acid, in the proportion of 1300 of the former to 100 of the latter, or in an atmosphere containing 7 or 8 per cent. of carbonic acid, and found that a number of *Minnows* introduced into the water so impregnated died within twenty-four hours, although 29 cubic inches were found by experiment to have taken up only 1 cubic inch of carbonic acid, which is in the ratio of 2.5 per cent.

Nevertheless it was afterwards found by a number of experiments, that other fish, such as Perch and Roach, would live in water which contained from 5 to 10 per cent. of carbonic acid, the larger of which quantities would be nearly double that which has been shown to be taken up by water under a pressure of 5 per cent. of the latter gas. On the other hand, where the

* As will appear by the following equation:—

Nitrogen	·76 × 0.42 = ·03192
Oxygen.....	·19 × 0.65 = ·01235
Carbonic acid	·05 × 1.06 = ·05300

·09727

quantity present might be estimated at 13 per cent. as compared to the volume of water, all the fish experimented upon speedily perished.

Nor was this merely the case with freshwater species, for I have had an opportunity within the last fortnight of repeating the same experiments at Ryde on certain sea-fish obtained off that coast. The species operated upon were those called Golden Maid (*Labrus*), two sorts of Pipe-fish (*Syngnathus*), Rock-fish (*Gobius niger*), Bull-fish (*Cottus scorpius*), and Flounder (*Platessa flesus*). Of these the Pipe-fishes and the Flounder remained alive for many hours in a tub of salt water containing 5 per cent. of carbonic acid, nor did they appear to suffer in consequence. When the amount was equal to 10 per cent., the Golden Maid (*Labrus*) was almost instantly affected, as were also the Pipe-fishes above operated upon.

Although therefore the difficulty of keeping sea-fish long alive in small quantities of salt-water, after they have been removed from their natural element, renders it more difficult to arrive at satisfactory results with them than with freshwater species, I think myself upon the whole warranted in concluding, that both kinds are equally tolerant of the smaller amount of carbonic acid, and alike susceptible of the poisonous influence of the larger.

Supposing however no error to exist in the calculation I have made above as to the amount of carbonic acid present in the water to which the minnows had been subjected, it will follow that whilst 5 per cent. is innoxious to some fish, 3 per cent. is noxious to others, and that the power of resisting its deleterious influence differs in different species. Nevertheless there seems reason for supposing, that an amount of carbonic acid in the atmosphere considerably larger than that which exists at present, would not communicate to the waters of the sea and rivers properties incompatible with the life of many fish.

Although reptiles are not supposed to have existed generally at so early a period as that of the carboniferous formation, yet as saurians have been detected in the coal-beds of Greensburg in Pennsylvania, and in those of Saar-bruck near Treves*, which are regarded as belonging to the same epoch, and as they made their appearance so abundantly in that which comes next to it in point of antiquity, it appeared worth while to ascertain what power of resisting the influence of carbonic acid might be possessed by the tribes now in being which belong to the same class of animals.

With reference however to this department of the inquiry, the experiments hitherto made by myself are far from numerous: I have however found, that frogs introduced under a bell-glass containing 5 per cent. of carbonic acid gas, appeared not to suffer, although they were killed when its proportion amounted to 10 per cent. Similar results were also obtained in experimenting upon newts; so that it would seem, as if, in accommodation to those arrangements of nature which were calculated to impart a greater luxuriance to the vegetation of the period alluded to, and to bring about during its continuance a larger accumulation of carboniferous matter, the lower tribes of animals, which at that time alone occupied the earth, were rendered less susceptible of the injurious influence of carbonic acid, than the higher orders subsequently created are found to be.

In conclusion then I may remark, that the general tenor of these experiments, so far as they have as yet gone, justifies us in inferring, that there is nothing in the organization of those plants and those animals of the present day, which appear most nearly allied to such as were in existence during the carbonife-

* See Lyell's Travels in America, 2nd Series.

rous epoch, or even somewhat subsequently to that period, militating against the probability, that a larger amount of carbonic acid may have been present in the atmosphere, and diffused through the waters of the sea and rivers, than is found, either in the one or in the other, at the present time; nor is there anything to prevent us from imagining, that the absorption of carbon by vegetables, and the consequent rapidity of their growth, may, at least within certain limits, have borne some proportion to the greater amount of carbonic acid assumed to have been present at earlier periods in the history of our globe, although whether this be actually the case, is a point which I hope to be able hereafter to settle more to my satisfaction, as well as to report the results arrived at on some future occasion.

Report on the Heat of Combination.

By THOMAS ANDREWS, *M.D., F.R.S., M.R.I.A.*

THERE are few molecular changes in the condition of matter which are not accompanied by the evolution or absorption of heat. The quantity of heat which is thus set free or absorbed, bears always a definite relation to the amount of the mechanical or chemical action, and its determination in each particular case is a problem of considerable interest as affording a measure of the forces in action. If we consider the great number of phenomena, mechanical, electrical and chemical, among which the production of heat forms the only bond of connexion which has hitherto been clearly ascertained, although there may be strong grounds for suspecting them to be only modified forms of the action of the same force, the importance of investigations of this kind to the future progress of physical science will become at once apparent.

The object of the present Report is to give a general view of the actual state of knowledge on the subject of thermo-chemistry, under which we may conveniently include a description of the thermal effects that occur in chemical actions of every kind. A few new experiments will be described in their proper places. These will be given in some detail, but when referring to experiments already published, all numerical quantities will, as far as possible, be avoided.

Before entering upon the consideration of chemical combinations and decompositions properly so called, it may be useful briefly to refer to the thermal changes which accompany solution. The earlier experiments on this subject having been made solely with the object of discovering frigorific mixtures, do not furnish quantitative measures of any scientific value. But of late years the inquiry has been pursued in a more useful way by Gay-Lussac, Thomson, Karsten, Chodnew and Graham. The salts examined have been chiefly the soluble sulphates, nitrates and chlorides, and the solvents pure water and saline and acid solutions. The principal results of these investigations I have endeavoured to express in the following propositions:—

1. The solution of a crystallized salt in water is always accompanied by an absorption of heat.
2. If equal weights of the same salt be dissolved in succession in the same liquid, the heat absorbed will be less on each new addition of salt.
3. The heat absorbed by the solution of a salt in water holding other salts dissolved, is generally less than that absorbed by its solution in pure water.
4. The heat absorbed by the solution of a salt in the dilute mineral acids, is generally greater than that absorbed by its solution in water.

As the subject is of great extent and the inquiry has hitherto embraced only a small number of cases of solution, it is not unlikely that some of these conclusions will require hereafter to be modified. From some experiments by Graham on the solution of salts belonging to certain isomorphous groups, there is reason to suspect the existence of a connexion between isomorphism and the absorption of heat in solution.

The foregoing remarks apply only to the solution of crystallized salts. If, however, we take a salt which crystallizes with water and make it anhydrous before solution, the thermal results will be altogether different. The anhydrous salt, when added to an excess of water, will first combine with its ordinary equivalent of water of crystallization, and the new compound will then dissolve. The change of temperature observed is therefore a complex quantity arising from the heat of combination due to the union of the anhydrous salt with water, and the heat absorbed by the solution of the hydrous salt. From a comparison of the results obtained on dissolving the same salt in the anhydrous and hydrous states, Graham has endeavoured to deduce the amount of heat due to the combination of the dry salt with its water of crystallization. According to his experiments, the sulphates of water, copper and manganese, disengage the same quantity of heat in combining with the first atom of water. The sulphates of magnesia and zinc also disengage equal quantities of heat in their complete hydration. The same simple relation is not however observed to hold between the quantities of heat evolved in the complete hydration of the first set of salts, or in the combination of the second set with the first atom of water. Neither does it apply to the other sulphates of the magnesian series.

None of the experiments hitherto published furnish all the requisite data for calculating with precision the absolute quantities of heat set free or absorbed in these cases of chemical action. The weights of the water and of the salt are given, and sometimes the weight and form of the vessel, and the material of which it is composed; but these data are not sufficient to enable us to deduce the true numbers from the observed increments or decrements of temperature. Knowing the weight and composition of the containing vessel, we may, it is true, calculate its thermal value in water. But other corrections, such as those for the heating and cooling influence of the surrounding air, can only be ascertained by special experiments performed under similar conditions to the original observations. Neither have any experiments of sufficient accuracy been made to determine the specific heats of the solutions formed.

To complete an investigation which would furnish all these elements, would be a work of very great labour, and will probably scarcely be undertaken till our instruments and means of observation are greatly improved. As a first step to such an inquiry, I may here describe a few preliminary experiments on the specific heats of some saline solutions, and on the quantities of heat absorbed in the solution of successive portions of the same salt.

To obtain results approaching to accuracy in experiments on the specific heats of saline solutions is extremely difficult, as the errors of experiment are often of nearly the same order of magnitude as the whole differences to be observed. The corrections for the cooling and heating action of the air and for the effects of radiation, cannot be estimated with any certainty by the application of general formulas founded on experiments made at a different time*; and the most careful examination of the calibre of the thermometer

* If the vessel be uncovered, changes in the hygrometric state of the atmosphere produce a very marked influence on the rate of cooling, when the excess of temperature above the air

tube will fail to render different parts of the scale accurately comparable with one another to a five-hundredth part. The general method pursued in the determination of the following specific heats was the same which I described some years ago*; but to avoid the uncertainties just referred to, alternate experiments were made with pure water and with the solution, under conditions as nearly as possible identical, and these were repeated till accurate means were obtained. By this mode of operating, a very great degree of precision may be given to experiments of this kind.

The only salts whose solutions have yet been examined are the nitrate of potash, the nitrate of soda and the chloride of sodium. They were all chemically pure. The density of each solution compared with water at the same temperature was also determined.

The first solution of nitrate of potash contained for every 100 parts of water 25.29 parts of the salt. The thermal values of the thermometer with large reservoir described in the paper already referred to, in terms of this solution and of water, were found in alternate experiments to be—

Solution I.	Water.
5044	4095
5047	4107
5050	4116
Mean.... 5047	4106

The temperature of the air during these experiments varied only from 18° C. to 18° 5 C.

The second and third solutions contained respectively 12.645 and 6.322 parts of nitrate of potash for 100 parts water. Air from 18° 5 to 18° 9.

Solution II.	Solution III.	Water.
4600	4393	4118
4620	4387	4105
4605	4385	4108
4610		
Mean.... 4610	4387	4110

From these data the specific heats of these solutions at the temperatures at which the experiments were performed, as compared with water at the same temperatures, may be easily computed. I have given them in the following table, as also the specific gravities of the liquids.

	I.	II.	III.
Specific heat	0.8135	0.8915	0.9369
Specific gravity	1.1368	1.0728	1.0382

The solutions of nitrate of soda contained 42.49, 21.245 and 10.622 parts respectively of nitrate of soda for 100 parts of water. The temperature of the air ranged from 17° 5 to 18° 8 during these experiments.

Solution I.	Water.	Solution II.	Water.	Solution III.	Water.
5261	4107	4775	4116	4499	4116
5234	5117	4782	4098	4498	4098
5247	4119	4787	4100	4488	4100
Mean.... 5247	4114	4781	4105	4495	4105

	I.	II.	III.
Specific heat	0.7838	0.8585	0.9131
Specific gravity	1.2272	1.1256	1.0652

amounts only to a few degrees; and even in a close apartment the increased agitation of the air on a windy day sensibly increases the rate of cooling.

* Philosophical Transactions for 1844, p. 34.
1849.

Of chloride of sodium two solutions were examined, the first containing 29·215, the second 14·607 chloride of sodium for 100 water. The air was nearly steady between 17°·9 and 18°.

Solution I.	Water.	Solution II.	Water.
5107	4111	4740	4111
5127	4106	4733	4106
5128		4731	
Mean....5121	4108	4735	4108
		I.	II.
Specific heat.....		0·8018	0·8671
Specific gravity.....		1·1724	1·0942

It may be not uninteresting to compare these numbers with those deduced by calculation from the specific heats of the salts in the dry state. The latter have been made the subject of experiment by Avogadro and Regnault, but their results do not agree well with each other. I have adopted Regnault's numbers in my calculations.

Solution.		Specific heat by experiment.	Mean spec. heat of dry salt and water.
Nitrate of potash	1.	0·8135	0·8463
" "	2.	0·8915	0·9145
" "	3.	0·9369	0·9566
Nitrate of soda	1.	0·7838	0·7847
" "	2.	0·8585	0·8736
" "	3.	0·9131	0·9307
Chloride of sodium	1.	0·8018	0·8224
" "	2.	0·8671	0·9000

It is obvious that the specific heat of the solution is, in every instance, less than the mean of the specific heats of its component parts, and that serious errors would be committed, if we should attempt to calculate on this principle of the thermal values of solutions which may be formed in the course of our experiments.

I have made a short series of experiments on the quantities of heat absorbed during the solution of nitrate of soda and of nitrate of potash, when added in successive portions to the same liquid. The results fully confirm those previously obtained by Graham, but as the experiments were only preliminary trials to a more extended investigation, it is not necessary to describe them in detail. I may briefly state, that on dissolving 12·22 grammes of nitrate of soda in 250 grammes of water and repeating the experiment with each new solution, till the water was nearly saturated, the following decrements of temperature were found:—

1.	2°·80 C.	6.	1°·60 C.
2.	2·43	7.	1·47
3.	2·11	8.	1·39
4.	1·89	9.	1·33
5.	1·75	10.	1·27
		11.	1·21

By the aid of the specific heats already determined, and knowing the thermal value of the vessel in which the experiments were performed (4·3 grms.), I have calculated for experiments 1, 4 and 9 the following numbers, which ex-

press the degrees Centigrade through which one part of water would be raised by the heat absorbed in the solution of one part of the salt.

1.	2.	3.
590	407	309

On dissolving 7.99 grms. nitrate of potash in 250 grms. water and repeating the operation as before, the successive decrements of temperature observed were,—

1.	2°65 C.	5.	2°06 C.
2.	2°49	6.	1°97
3.	2°34	7.	1°87
4.	2°22	8.	1°75

Combination of Sulphuric Acid with Water.—In an elaborate memoir on thermo-chemistry, which was published in Poggendorff's 'Annalen,' Hess made the first systematic attempt to reduce the quantities of heat disengaged in the formation of the hydrates of sulphuric acid to definite laws. His experiments were made by two distinct methods, which however did not give exactly the same results. In the first or indirect method of operating, equivalent quantities of SO_3 , $\text{SO}_3 \text{HO}$, $\text{SO}_3 2\text{HO}$, &c., were respectively mixed with a large excess of water and the increments of temperature observed in each case. The difference between the increments observed on mixing any two compounds with water, was assumed to correspond to the heat due to the combination of the first compound with the number of equivalents of water necessary to convert it into the second. Thus, if $\text{SO}_3 \text{HO}$ added to $x \text{HO}$ gave a units of heat, and $\text{SO}_3 3\text{HO}$ added to the same $x \text{HO}$ gave b units, $a-b$ was supposed to represent the number of units which would be obtained on combining $\text{SO}_3 \text{HO}$ and 2HO . In the second, or direct method, each compound was combined with the quantity of water exactly required to convert it into the succeeding compound, and the heat measured by observing the increment of temperature of a determinate quantity of water surrounding the vessel in which the combination took place. These experiments have since been repeated by Graham, Abria, and Fabre and Silbermann, but their results do not generally agree with the statements of Hess.

The fundamental principle laid down by the latter is, that there exists a simple relation between the numbers which express the quantities of heat set free in the formation of the successive hydrates of sulphuric acid. If we designate by $2a$ the heat disengaged in the combination of $\text{SO}_3 \text{HO}$ with HO , then, according to Hess, the heat set free in the formation of the other hydrates will be

$\text{SO}_3 + \text{HO}$	$8a$
$\text{SO}_3 \text{HO} + \text{HO}$	$2a$
$\text{SO}_3 2\text{HO} + \text{HO}$	a
$\text{SO}_3 3\text{HO} + 3\text{HO}$	a
$\text{SO}_3 6\text{HO} + x\text{HO}$	a

In an early part of his memoir, Hess gives 38.85 for the value of a , but this he afterwards changes to 46.94, still maintaining however the accuracy of the ratios. It is difficult to see how this can be correct. The only experiment described by Hess on the combination of the anhydrous acid with water gave the number 305, which bears to 46.94, not the ratio of 8 : 2, but nearly that of 6.5 to 2. Abria obtained a still lower number for the combination SO_3 with HO . There can therefore be little doubt, if the experiments may be relied on, that the first ratio is too high. It remains to be seen how far the others have been confirmed by subsequent investigations.

The multipliers of a for the three latter combinations given in the preceding table are, according to Graham's experiments, 0.72, 1.35 and 1.18. These numbers agree with Hess's statement only so far as to indicate that the heat evolved in the combination of SO_3 HO with HO is nearly the same as that evolved in the combination of SO_3 2HO with 4HO.

The experiments of Abria were performed by the direct method and with a similar apparatus to that employed by Hess. Adopting the views of Hess as to the quantities of heat in the cases of combination being in simple relations to one another, he arrives nevertheless at very different numbers for the ratios. In the next table I have given Abria's theoretical whole numbers, as also the exact numbers which result from his experiments.

	Theory.	Experiment.
$\text{SO}_3 + \text{HO} \dots\dots\dots$	$6a \dots\dots\dots$	$6.02a$
$\text{SO}_3 \text{ HO} + \text{HO} \dots\dots\dots$	$2a \dots\dots\dots$	$2.00a$
$\text{SO}_3 \text{ 2HO} + \text{HO} \dots\dots\dots$	$a \dots\dots\dots$	$0.95a$
$\text{SO}_3 \text{ 3HO} + \text{HO} \dots\dots\dots$	$\frac{4}{3}a \dots\dots\dots$	$0.57a$
$\text{SO}_3 \text{ 4HO} + \text{HO} \dots\dots\dots$	$\frac{2}{3}a \dots\dots\dots$	$0.35a$
$\text{SO}_3 \text{ 5HO} + \text{HO} \dots\dots\dots$	$\frac{2}{3}a \dots\dots\dots$	$0.22a$

In the three latter cases, the simple relations in the second column are scarcely borne out by the experimental numbers. The only agreement with the ratios given by Hess is in the combination SO_3 2HO with HO, which, according to both experimenters, sets free exactly half as much heat as the combination SO_3 HO with HO. The value of a , given by Abria, is 39.33.

The latest experiments on this subject are those of Fabre and Silbermann, from which I have calculated the following multipliers for a :—

$\text{SO}_3 \text{ HO} + \text{HO} \dots\dots\dots$	$2.00a$
$\text{SO}_3 \text{ 2HO} + \text{HO} \dots\dots\dots$	$0.93a$
$\text{SO}_3 \text{ 3HO} + \text{HO} \dots\dots\dots$	$0.53a$
$\text{SO}_3 \text{ 4HO} + \text{HO} \dots\dots\dots$	$0.32a$
$\text{SO}_3 \text{ 5HO} + \text{HO} \dots\dots\dots$	$0.26a$

Hess has also attempted to express by simple multiple relations the quantities of heat disengaged in the formation of the hydrates of nitric acid, but for the details of his results I must refer to the original memoir.

Combination of Acids and Bases.—In the same memoir Hess describes an extensive set of experiments on the heat evolved during the union of certain bases with acids of different degrees of concentration. These experiments serve to illustrate the general principle, that in the formation of a chemical compound the heat developed is a constant quantity, being the same in amount, whether the combination takes place directly at one time or indirectly at repeated times. Thus he finds that on neutralizing an aqueous solution of ammonia with sulphuric acid, containing one, two, three and six atoms of water, there is a different development of heat in each case; but by adding to the results found by experiment in the three latter cases the quantities of heat due to the combination of the monohydrated acid, with one, two and five atoms of water respectively, the same number is obtained in each case as in the direct combination of the monohydrated acid itself. This principle is correct, but it is almost self-evident and scarcely required so elaborate a proof.

The bases examined by Hess were potash, soda, ammonia and lime, which he combined in different ways with the sulphuric, nitric and hydrochloric acids. The conclusion at which he arrives is, that the same acid in combining with equivalents of different bases produces the same quantity of heat, but at the same time he expresses some doubt as to the applicability of this principle

to all similar cases of combination. Indeed his own experiments with lime and ammonia do not accurately agree with it; I refer particularly to his experiments with ammonia, which, when properly interpreted, appear to me to prove clearly that that base in combining with acids develops less heat than potash or soda, although I am aware that Hess himself has drawn from them a different conclusion.

About the time of the publication of the first part of Hess's memoir, I had completed an investigation of the same subject, but instead of employing strong solutions of the acids and bases, I diluted all the liquids largely with water previous to examining their thermal reactions. In this way I hoped to avoid the complex effects that arise when successive combinations and decompositions of different kinds occur in the same chemical action, and the result fully realized my anticipations. The general conclusion deduced from this investigation may be briefly expressed, by stating that *the heat developed during the union of acids and bases is determined by the base and not by the acid*. The following special laws will be found to comprehend the greater number of cases of chemical action to which the foregoing principle can be made to apply.

1. An equivalent of the same base, combined with different acids, produces *nearly* the same quantity of heat.
2. An equivalent of the same acid, combined with different bases, produces different quantities of heat.
3. When a neutral salt is converted into an acid salt by combining with one or more equivalents of acid, no disengagement of heat occurs.
4. When a double salt is formed by the union of two neutral salts, no disengagement of heat occurs.
5. When a neutral salt is converted into a basic salt, the combination is accompanied by the disengagement of heat.
6. When one and the same base displaces another from any of its neutral combinations, the heat evolved or absorbed is always the same whatever the acid element may be.

As some of the bases (potash, soda, barytes and strontia) form what we may perhaps designate an isothermal group, such bases will develop the same, or nearly the same heat in combining with an acid, and no heat will be developed during their mutual displacements.

These laws are not intended to embrace the thermal changes which occur during the conversion of an anhydrous acid and base into a crystalline compound. The steps by which such a conversion is effected are generally very complicated, and involve successive combinations and decompositions. We cannot combine, at ordinary temperatures, a dry acid and a dry base; and when combination takes place in presence of water, hydrates of the acid and base are first formed, which are afterwards decomposed, and the crystalline salt finally obtained is sometimes anhydrous, sometimes combined with water. To expect simple results where so many different actions must produce each its proper thermal effect, would be altogether vain, and to introduce the consideration of some of these actions without the whole would only render the numbers empirical. In the experiments from which the foregoing laws were deduced, the acids and bases before combination, and the compounds after combination, were as nearly as possible in the same physical state. The only change which occurred was the combination of the acid and base, and the heat evolved must therefore have arisen from the act of combination. Such changes of temperature as are produced by solution are not in any way concerned in producing these thermal effects, as none of the reacting bodies assumed at any time the solid state. The insoluble bases form, it is true, an

unavoidable exception to this statement, and in the experiments with them, the results would require to be corrected for the heat due to the change of the base from the solid to the fluid state. As this correction, however, although unknown, must be a constant quantity for the same base, it would not, if applied, interfere with the direct proof of the first law.

In an inquiry of this kind, it is important, while endeavouring to generalize the results of experiment, to point out at the same time the differences which occur in particular cases between those results and the numbers deduced from the theory. In the whole range of the science of heat, scarcely a single general principle has yet been discovered which is strictly in accordance with all the results of experiment; and from the application of improved methods of experimenting, discrepancies of this kind have of late years been found to exist where they had not before been suspected.

In the original experiments from which the first of the foregoing laws was deduced, the mean heat developed by the nitric, phosphoric, arsenic, hydrochloric, hydriodic, boracic, chromic and oxalic acids being $6^{\circ}61$, the greatest deviation from the mean on either side amounted only to $0^{\circ}15$; and a similar remark may be made with respect to the combinations of soda, barytes and ammonia. On the other hand, sulphuric acid disengaged about $0^{\circ}7$ more than the mean quantity, and the citric, tartaric and succinic acids about $0^{\circ}5$ less. To ascertain whether these discrepancies depended on the state of dilution of the solutions, I repeated these experiments lately with solutions of only half the strength, but although only half the heat was obtained, similar differences were still found to exist. If, instead of taking just the quantity of sulphuric acid required to neutralize the base, we employ a large excess, the heat given out during combination will be nearly $0^{\circ}2$ less, which reduces the anomaly presented by this acid to about $0^{\circ}5$. The sulphurous acid not having been formerly examined, I have lately made some experiments on its thermal relations to the bases, the results of which are very interesting. Although one of the feeblest acids, it agrees almost exactly with sulphuric acid in the heat developed by its combination with potash. In several carefully conducted experiments the increments of temperature did not differ more than $0^{\circ}05$. Combining this with the fact that acids differing so much in composition and properties as the nitric, boracic and oxalic, also disengage almost exactly the same amount of heat in the act of combination, there will, I conceive, be little hesitation in attributing the deviations already mentioned to the influence of extraneous causes, and in acknowledging the truth of the principle, that the heat of combination depends upon the neutralization or combination of the base, and not upon the nature of the acid by which the base is neutralized. That other causes of change of temperature, of feeble power, do actually exist, may be proved by the following fact. If we add an excess of sulphuric acid to the neutral solution after combination has taken place, a slight fall of temperature, amounting to about $0^{\circ}1$, will occur; if we make the same experiment with sulphurous acid, an increase of temperature of about equal amount will be observed, while with oxalic acid there will be no thermal change of any kind. Now it is very probable that the same causes which produce these slight thermal effects are in operation during the original combination of the acid and base, and if so, they would introduce anomalies into the quantities of heat then developed.

There is one important condition, which, as far as my investigations extend, requires to be fulfilled in order that the first law may hold good; viz. the acid must have the power of neutralizing the alkaline reaction of the bases. It is for this reason that the hydrocyanic, carbonic and arsenious acids do not develop the same quantity of heat in combining with potash as

the other acids. The sparing solubility of the arsenious acid in water prevents an accurate examination of its thermal reactions; but on repeated trials I obtained $0^{\circ}25$ F., on combining with it the same quantity of potash which under similar conditions gave $0^{\circ}34$ with nitric acid. Although a considerable excess of arsenious acid was taken, as proved by the fact that further additions produced no new development of heat, the solution still exhibited an alkaline reaction. The same is also well known to be true of the hydrocyanic and carbonic acids. In the case of bases, such as the oxide of copper, whose salts have all an acid reaction, this criterion will not apply; but the exceptional acids are so few, and their peculiarities so well-marked, that they give rise to little difficulty in the experimental investigation.

The quantities of heat developed by different bases in combining with the same acid are so different, that it is unnecessary to refer particularly to the proofs of the second law. In this case, neutralizing power has no apparent influence on the results, as oxide of silver, which forms salts neutral to test paper with the strongest acids, is one of the feeblest bases if measured by its thermal power. It develops, in fact, little more than one-third of the heat which potash does in combining with the acids.

The more recent experiments of Graham and of Fabre and Silbermann, confirm the accuracy of the facts from which the second and third laws were deduced, that no heat is developed on mixing solutions of neutral salts or of a neutral salt and acid*. It is difficult however to obtain, as Graham has remarked, positive proof of the occurrence of combination, when such solutions are brought into contact. Fabre and Silbermann indeed are of opinion that acid salts cannot exist in the state of solution.

Double Decompositions.—When solutions of two neutral salts are mixed and a precipitate formed from their mutual decomposition, there is always a disengagement of heat, which, though not considerable, is perfectly definite in amount. It does not altogether arise from the components of the precipitate having changed from the fluid to the solid state—as it is not always the same for the same precipitate—but it is chiefly connected with the latent heat of the precipitate. If the latter contains water of crystallization, the heat given out is much greater than when an anhydrous precipitate is formed. Experiments of this kind appears at first view to be extremely simple, but it is often difficult to obtain exact results, from the length of time during which the heat continues to be disengaged, even when the combination is aided by brisk agitation.

The precipitation of the salts of barytes and lead by a soluble sulphate appeared to present favourable conditions for investigation, and accordingly I made an extensive set of experiments with these classes of salts. This is indeed the only part of the inquiry which I have been able to complete. A few other examples of double decomposition will however be noticed.

Chloride of Barium and Sulphate of Magnesia.—Of chloride of barium carefully purified and dried immediately before the experiment at a low red heat, 16.94 grms. were taken in each experiment, equivalent to 19.00 grms. sulphate of barytes. The weight of sulphate of magnesia (dry) was 10.3 grms., which is a little more than sufficient to decompose completely the chloride of barium. The entire weight of the water employed to dissolve the salts was 234 grms., of which one-third was taken to dissolve the sulphate of magnesia, and two-thirds to dissolve the chloride of barium. The solutions were contained in vessels of thin copper, the smaller of which, when filled with its

* Slight changes of temperature may however occasionally be detected; but in some cases a development, in others, an absorption of heat occurs. These thermal effects evidently arise from causes altogether distinct from those which produce the combination of acids and bases.

solution, floated in the larger, and could be rapidly rotated, so as to produce in a short time a perfect equilibrium of temperature throughout the whole apparatus. The thermometer attained a maximum about 8' after the solutions were mixed. I have elsewhere indicated the precautions to be taken in such experiments, and shall therefore not refer to them here. In the following statements, I have given the temperature of the air, the increment actually observed in Centigrade degrees, and the number of degrees through which 1 grm. of water would be raised by the precipitation of 1 grm. and 1 equiv. (oxygen = 1) of the precipitate. In calculating the latter numbers, all the usual corrections were applied to the observed increments of temperature:—

Temperature of air	18 ^o ·3	14 ^o ·4
Increments observed	1·95	1·96
Heat for 1 grm. BaO, SO ₃	25·4	25·2
Heat for 1 equiv. BaO, SO ₃ ..	368·9	

Chloride of Barium and Sulphate of Soda.—The same weight of chloride of barium taken as before, and an equivalent weight of sulphate of soda.

Temperature of air	20 ^o ·2	18 ^o ·7
Increments observed	1·57	1·55
Heat for 1 grm. BaO, SO ₃	20·4	20·1
Heat for 1 equiv. BaO, SO ₃ ..	294·5	

Chloride of Barium and Sulphate of Zinc.

Temperature of air	19 ^o ·7	19 ^o ·6
Increments observed	1·69	1·72
Heat for 1 grm. BaO, SO ₃	22·2	22·4
Heat for 1 equiv. BaO, SO ₃ ..	325·1	

Chloride of Barium and Protosulphate of Iron.

Temperature of air	18 ^o ·8	
Increment observed	1·99	
Heat for 1 grm. BaO, SO ₃	25·6	
Heat for 1 equiv. BaO, SO ₃	373·2	

Chloride of Barium and Sulphate of Copper.

Temperature of air	17 ^o ·5	17 ^o ·6
Increments observed	1·85	1·85
Heat for 1 grm. BaO, SO ₃	24·7	24·6
Heat for 1 equiv. BaO, SO ₃ ..	359·4	

Chloride of Barium and Sulphate of Ammonia.

Temperature of air	11 ^o ·3	11 ^o ·1
Increments observed	1·85	1·84
Heat for 1 grm. BaO, SO ₃	24·2	24·1
Heat for 1 equiv. BaO, SO ₃ ..	352·1	

Nitrate of Barytes and Sulphate of Magnesia.—As the nitrate of barytes is sparingly soluble in water, 10·6 grms. only were taken, which is equivalent to half the quantity of chloride of barium used in the foregoing experiments. The other salts were reduced in the same proportion.

Temperature of air	13 ^o ·9	14 ^o ·4
Increments observed	0·82	0·82
Heat for 1 grm. BaO, SO ₃	22·2	21·2
Heat for 1 equiv. BaO, SO ₃ ..	316·4	

Nitrate of Barytes and Sulphate of Soda.

Temperature of air	14.4	
Increment observed	0.75	
Heat for 1 grm. BaO, SO ₃	20.5	
Heat for 1 equiv. BaO, SO ₃		298.9

Nitrate of Barytes and Sulphate of Zinc.

Temperature of air	13.9	14.1
Increments observed	0.83	0.83
Heat for 1 grm. BaO, SO ₃	22.0	22.0
Heat for 1 equiv. BaO, SO ₃ ..	320.7	

Nitrate of Barytes and Sulphate of Copper.

Temperature of air	14.4	14.4
Increments observed	0.88	0.91
Heat for 1 grm. BaO, SO ₃	23.0	24.5
Heat for 1 equiv. BaO, SO ₃ ..	346.2	

The salts of lead were next examined. The precipitation of the sulphate of lead took place with the same facility as that of the sulphate of barytes, the thermometer attaining the maximum in eight minutes.

Acetate of Lead and Sulphate of Magnesia.—The acetate of lead was pure and in crystals, 4.17 grms. precipitated by oxalate of ammonia gave 2.454 grms. oxide of lead, which exactly agrees with the theoretical composition of the salt. In each of the following experiments, 30.80 grms. acetate of lead were taken, corresponding to 24.63 sulphate of lead:—

Temperature of air	12.7	12.3
Increments observed	1.01	0.97
Heat for 1 grm. PbO, SO ₃	9.9	9.9
Heat for 1 equiv. PbO, SO ₃ ..	187.6	

Acetate of Lead and Sulphate of Soda.

Temperature of air	12.3	12.2
Increments observed	0.84	0.86
Heat for 1 grm. PbO, SO ₃	8.3	8.5
Heat for 1 equiv. PbO, SO ₃ ..	159.2	

Acetate of Lead and Sulphate of Zinc.

Temperature of air	12.3	13.9
Increments observed	0.41	0.37
Heat for 1 grm. PbO, SO ₃	4.1	3.7
Heat for 1 equiv. PbO, SO ₃ ..	73.9	

In the last experiment the precipitation was so slow that the thermometer did not attain the highest point for thirteen minutes after the solutions were mixed.

When the salts of lead are precipitated by a neutral oxalate, the heat engaged is much greater than when they are precipitated by a sulphate. I have not examined in detail the increments of temperature in this class of precipitations, but in one experiment, in which the acetate of lead was precipitated by the oxalate of potash, 36.2 units of heat were obtained for each gramme of oxalate of lead.

In the experiments next to be described, a dilute acid was substituted for one of the neutral solutions.

Chloride of Barium and Sulphuric Acid.—The same quantities of chloride of barium and of water were taken as in the experiments with the neutral sulphates. A slight excess of sulphuric acid was employed to secure complete precipitation.

Temperature of air	17°·8	18°·4	15°·1	9°·8
Increments observed	3°·44	3°·46	3°·38	3°·42
Heat for 1 grm. BaO, SO ₃	45°·6	45°·6	44°·0	44°·2
Heat for 1 equiv. BaO, SO ₃			654°·6	

Nitrate of Barytes and Sulphuric Acid.—As in the former experiments, half the usual equivalents only were taken.

Temperature of air	15°·0	15°·3
Increments observed	1°·50	1°·49
Heat for 1 grm. BaO, SO ₃	40°·4	39°·2
Heat for 1 equiv. BaO, SO ₃ ..		580°·2

Acetate of Barytes and Sulphuric Acid.—Half equivalents were taken in this case also.

Temperature of air	12°·3	12°·5
Increments observed	1°·90	1°·91
Heat for 1 grm. BaO, SO ₃	49°·5	49°·3
Heat for 1 equiv. BaO, SO ₃ ..		720°·2

Acetate of Barytes and Oxalic Acid.—11·2 grms. of acetate of barytes and 5·33 grms. oxalic acid taken.

Temperature of air	12°·3	12°·8
Increments observed	1°·19	1°·19
Heat for 1 grm. BaO, C ₂ O ₃ ..	22°·1	21°·8
Heat for 1 equiv. BaO, C ₂ O ₃ ..		309°·0

Acetate of Lead and Sulphuric Acid.—Of the acetate 30·8 grms. taken and an equivalent of the acid.

Temperature of air	14°·9	14°·1
Increments observed	2°·84	2°·86
Heat for 1 grm. PbO, SO ₃	28°·0	29°·2
Heat for 1 equiv. PbO, SO ₃ ..		542°·0

Nitrate of Lead and Sulphuric Acid.—Of nitrate of lead 26·26 grms. taken.

Temperature of air	9°·8	10°·3
Increments observed	1°·63	1°·66
Heat for 1 grm. PbO, SO ₃	16°·3	16°·4
Heat for 1 equiv. PbO, SO ₃ ..		309°·8

Acetate of Lead and Oxalic Acid.—15·4 grms. of acetate of lead were taken.

Temperature of air	9°·8
Increment observed	2°·12
Heat for 1 grm. PbO, C ₂ O ₃	4°·3
Heat for 1 equiv. PbO, C ₂ O ₃	792°·9

These experiments can only be regarded as introductory to an extended and interesting subject of inquiry. With such limited data, it would be premature to attempt to draw any general inferences.

Solution of Metals in Nitric Acid.—Every chemist is familiar with the

violent action of nitric acid on zinc and copper, and the abundant evolution of gas which accompanies it. But the facility with which the gases may be condensed by the acid solution is probably not so generally known, and when the experiment is made for the first time cannot fail to excite surprise. If a small vessel of thin German glass, of about the capacity of half a fluid ounce, be half-filled with nitric acid of density 1·4, and a slip of zinc be suspended in the upper part so as not to touch the acid, the flask hermetically sealed, and finally inverted while surrounded with cold water, a very violent action will occur, but without bursting the vessel. Having ascertained these facts, there was little difficulty in measuring the heat disengaged during the solution of the metals in nitric acid. The metal was weighed in a glass tube open at one end, which was introduced into a thin glass vessel containing nitric acid of specific gravity 1·4. The latter was then carefully closed and introduced into a copper vessel filled with water, and suspended in a metallic cylinder which was capable of rotation. On inverting the apparatus, the metal and acid came into contact, and the solution was completed in a few seconds. The rotation was afterwards continued for five minutes, which was sufficient to diffuse the heat disengaged through every part of the calorimeter.

Solution of Zinc in Nitric Acid.

	I.	II.	III.	IV.
Temperature of air	4·5	6·2	8·0	5·8
Increment found ..	2·66	2·78	2·83	2·71
Increment corrected	2·65	2·77	2·82	2·71
Weight of zinc	0·587 grm.	0·600 grm.	0·615 grm.	0·604 grm.
Weight of water ..	294·8	284·4	289·3	294·6
Value of acid	7·4	6·9	6·5	6·6
Value of vessels	14·3	14·3	14·3	14·3
Heat of combination	1429	1411	1422	1420

Hence we have for the heat disengaged during the solution in nitric acid of—

1 grm. zinc	1420
1 equiv. zinc	5857

Solution of Copper in Nitric Acid.

	I.	II.	III.	IV.
Temperature of air ..	8·9	6·8	7·8	8·5
Increment found	2·56	2·58	2·58	2·57
Increment corrected ..	2·55	2·56	2·57	2·56
Weight of copper	1·202 grm.	1·204 grm.	1·206 grm.	1·213 grm.
Weight of water	274·2	273·2	273·3	275·4
Value of acid	14·5	16·8	15·6	15·5
Value of vessels	16·8	16·8	16·8	16·8
Heat of combination ..	648	652	651	650

We have therefore for the heat disengaged during the solution in nitric acid of—

1 grm. copper	650
1 equiv. copper	2578

I made several attempts to determine the amount of heat disengaged in the solution of iron in nitric acid, but although acids of different strengths were employed, I was unable to obtain satisfactory results, as the iron always assumed the passive state before a sufficient quantity was dissolved to raise the temperature of the water in the calorimeter through 1°. Silver, bismuth

and other metals were also tried, but the solution did not proceed with sufficient energy.

The numbers 5857 and 2578 obtained above, are very nearly in the same ratio as 5366 and 2394, which, according to my experiments (and their results differ little from those of Dulong), express the quantities of heat set free by the combustion of zinc and copper in oxygen gas. This shows clearly that the oxidation of the metals is the principal cause of the heat produced during their solution in nitric acid. Other causes of thermal change however exist, which must exercise a considerable influence. Such are the combinations of the oxide with the nitric acid, the separation of the elements of a portion of the nitric acid during the solution, and the condensation of the oxygen gas during the combustion. From these and other circumstances, it is not unlikely that the numbers expressing the quantities of heat disengaged in these reactions will not be found in all other cases to be so nearly in the same ratio as in the foregoing examples; but it may be presumed that the general results will be the same, and that those metals which produce a greater amount of heat by their combustion in oxygen will also produce a greater amount of heat when dissolving in nitric acid.

The heat produced by the solution of copper in nitromuriatic acid is, according to the result of a single trial, about $\frac{1}{7}$ th less than that produced by its solution in nitric acid.

Metallic substitutions.—I have lately treated this part of the subject at so great length in a paper published in the Philosophical Transactions, that I shall here only transcribe the general result of the investigation. It is thus expressed:—"When an equivalent of one and the same metal replaces another in a solution of any of its salts of the same order, the heat developed is always the same; but a change in either of the metals produces a different development of heat." This is evidently an analogous law to that already stated for the thermal changes which accompany basic substitutions. The numerical results are however entirely different in their details.

Combustions in Oxygen Gas.—Since the time when Lavoisier published his celebrated experiments on the heat produced by combustion, the subject has frequently engaged the attention of chemists. But few results were obtained of any scientific value, till the posthumous publication of Dulong's valuable researches, which have formed the basis of all subsequent inquiries. More recently, Grassi and Fabre and Silbermann have examined the same subject, and I have myself lately published a set of experiments upon it, which were made some years ago. With the exception of some of Grassi's results, the numbers obtained by the different experimenters agree very nearly with each other, and we may therefore consider the quantities of heat developed by the combination of oxygen with the more important simple bodies and with some of their compounds to be determined with considerable precision. Fabre and Silbermann have also examined the combustion of carbon in the protoxide of nitrogen. A tabular view of nearly all the numerical results hitherto obtained, will be found in the edition of Gmelin's Hand-book of Chemistry recently published by the Cavendish Society. I shall here therefore confine myself to a few general observations.

The following bodies in their ordinary physical states, viz. hydrogen, carbonic oxide, cyanogen, iron, tin and antimony, disengage nearly the same amount of heat in combining with an equal volume of oxygen. The numbers which express the heat of combination in these cases do not in fact differ from one another more than $\frac{1}{40}$ th part of the whole quantity,—a difference which is nearly within the limit of the errors of experiment. This observation applies only to the quantities of heat actually obtained by experiment.

But if we apply corrections for the heat due to the changes of physical state which occur in some of these reactions, the same agreement will no longer be observed. Thus in the combustion of carbonic oxide, the resulting compound is obtained in the gaseous state, while in the combustion of hydrogen it is condensed during the course of the experiment into a liquid; and if, from the entire quantity of heat evolved in the latter case, we deduct that arising from the condensation of the vapour of water, the result will no longer agree with the quantity of heat obtained in the former case. Protoxide of tin may probably be added to the foregoing list, and perhaps also phosphorus, which disengages however a little more heat than the other bodies.

Sulphur, copper and the protoxide of copper, disengage, during their combustion in oxygen gas, a little more than half the quantity of heat evolved by the preceding class of bodies. Carbon occupies an intermediate position, while zinc gives out a larger quantity of heat than any of the bodies already enumerated; and potassium a still larger quantity than zinc. The combustion of a large number of carbo-hydrogens, alcohols, æthers and organic acids has been examined by Fabre and Silbermann. Their results prove the opinion to be erroneous, that if we subtract the oxygen in the form of water, the remaining elements give the same amount of heat as in the free state.

In the reduction of oxide of iron by hydrogen gas, no perceptible evolution of heat occurs, while in the reduction of the oxide of copper by the same gas, it is well known that ignition takes place, unless the experiment is conducted very slowly. These phænomena are at once explained by the fact, that in combining with oxygen, hydrogen gas disengages nearly the same quantity of heat as iron, and twice as much heat as copper.

Fabre and Silbermann have observed that the heat of combustion is influenced to a considerable extent by the physical state in which the combustible exists before combination. According to their experiments, carbon in the form of the diamond disengages 7824 units of heat during its combustion in oxygen gas; in the form of graphite 7778 units; and in that of wood-charcoal 8080 units. According to my own experiments and those of Despretz, the combustion of wood-charcoal produces only about 7900 units. Fabre and Silbermann have also supposed that they were able to detect differences in the quantities of heat disengaged by sulphur in its different allotropic states. The same chemists have also made the remarkable observation, that a much larger quantity of heat is evolved by the combustion of carbon in the protoxide of nitrogen than in oxygen gas. From this it should follow that in the separation of the elements of the protoxide of nitrogen, heat would be set free. Accordingly, by passing the protoxide of nitrogen through a platina tube heated to redness by burning charcoal in a suitable apparatus, it was found that a larger quantity of heat was actually evolved than could be accounted for by the weight of charcoal burned.

Combustions in Chlorine Gas.—Some years ago, I published the results of an investigation on the quantities of heat evolved in the combination of zinc and iron with chlorine, bromine and iodine; and I have lately given an account of a set of experiments on the combustion of potassium, tin, antimony, mercury, phosphorus and copper in chlorine gas. So far as I am aware, the only other experiments on this subject are those described by M. Abria on the combustion of hydrogen and phosphorus in chlorine. From a comparison of the results, it appears that in several cases the quantities of heat evolved during the combustion of the same metal in oxygen and chlorine are nearly the same. This observation applies particularly to the cases of iron, tin and antimony. Zinc however disengages a greater quantity of heat with chlorine (6309 units) than with oxygen (5366 units), and copper nearly twice

as much (3805 and 2394 units). Phosphorus, on the contrary, gives less heat with chlorine than with oxygen (2683 and 4509 units). On comparing the quantities of heat disengaged by different bodies in combining with the same volume of chlorine, it will be found that potassium disengages a larger amount of heat than any other body hitherto examined, twice as much as zinc, and nearly four times as much as tin, antimony or copper.

Combinations of Bromine and Iodine.—The heat disengaged by the same body in combining with bromine is less than with chlorine, and with iodine less than with bromine. The greater development of heat in the case of chlorine is at least partly due to that element being in the gaseous state before combination. In some early experiments, I observed that the quantities of heat developed on converting equivalent solutions of the sesquichloride, sesquibromide and sesquiiodide of iron into the corresponding proto-compounds were equal. When a solution of protochloride of iron is converted into sesquichloride by agitation with chlorine gas, a definite disengagement of heat occurs, as also in the formation of the sesquibromide of iron by the combination of the protobromide and bromine; but in the corresponding reaction between the protoiodide of iron and iodine, no change of temperature can be observed.

Report of the Committee on the Registration of the Periodic Phænomena of Plants and Animals, consisting of EDWIN LANKESTER, M.D., Mr. R. TAYLOR, Mr. W. THOMPSON, Rev. L. JENYNS, Prof. HENSLOW, Mr. A. HENFREY, Sir W. C. TREVELYAN, Bart., and Mr. PEACH.

SINCE the last Meeting of the Association, your Committee have made several alterations in the Tables for the purpose of registering the periodic phænomena occurring in plants and animals, which were then submitted for the approval of the members. These tables have been sent to upwards of fifty members of the Association and others, who have undertaken to observe.

But few of these tables have yet been returned to the Committee, but they hope at the next meeting to find more abundant fruit of their labours. They have to acknowledge, however, the receipt of a very complete registration of the periodic phænomena of the plants and animals in the neighbourhood of Swansea, by Matthew Moggridge, Esq.; also observations on periodic phænomena for 1848, at Polpero in Cornwall, by J. E. Couch, Esq.; a list of the visitation and departure of birds at Llanrwst in Wales, by J. Blackwall, Esq.; and observations on the foliation and defoliation of plants, by T. L. Lloyd, Esq.

Ninth Report of a Committee, consisting of H. E. STRICKLAND, Prof. DAUBENY, Prof. HENSLOW, and Prof. LINDLEY, appointed to continue their Experiments on the Growth and Vitality of Seeds.

DURING the past summer, a portion of each kind of seed collected in 1841 and 1846 were resown at Oxford and Chiswick, together with a few other kinds contributed by Miss Molesworth, of Cobham Lodge, Surrey.

Those forwarded to Cambridge arrived just after Mr. Murray (Curator of the Botanic Garden there) had started for a botanical tour in the north, and he did not receive them till his return, when it was too late this year to have them sown. A statement respecting them will therefore be given in the Report for 1850.

We again beg to remind persons interested in these experiments, that we shall be glad to receive contributions of seeds of known date, whether old or new, especially those of genera not named in the List submitted to the Meeting of the Association in 1848.

The results obtained will be seen by reference to the following Table:—

Name and Date when gathered.	No. sown.	No. of Seeds of each Species which vege- tated at			Time of vegetating in days at			Remarks.
		Ox- ford.	Cam- bridge.	Chis- wick.	Ox- ford.	Cam- bridge.	Chis- wick.	
1841.								
1. <i>Vicia sativa</i>	50		8		26	
2. <i>Daucus Carota</i>	100	1						
3. <i>Cannabis sativa</i>	50	3	10	9	14	
4. <i>Pastinaca sativa</i>	100							
5. <i>Brassica Rapa</i>	300	7	8	11	14	
6. <i>Linum usitatissimum</i>	150	12	6	8	14	
7. <i>Lepidium sativum</i>	100	14	5	10	14	
8. <i>Polygonum Fagopyrum</i>	50	1	6		14	
9. <i>Phalaris canariensis</i>	100	6	13	12	26	
10. <i>Brassica Napus</i>	150		4			14	
11. <i>Carum Carui</i>	200		2			26	
12. <i>Petroselinum sativum</i>	50	1						
13. <i>Trifolium</i> , sp.....	150							
14. <i>Lactuca sativa</i>	50							
15. <i>Brassica oleracea</i>	50							
16. <i>Pisum sativum</i>	50	3	12	16	14	
17. <i>Faba vulgaris</i>	25	18	22	9		14	
18. <i>Phaseolus multiflorus</i>	25	1						
19. <i>Triticum æstivum</i>	100							
20. <i>Hordeum vulgare</i>	100							
21. <i>Avena sativa</i>	100	24	13	15	26	
22. <i>Æthusa Cynapioides</i>	100	1						
23. <i>Antirrhinum majus</i>	300							
24. <i>Calendula pluvialis</i>	200							
25. <i>Collinsia heterophylla</i>	300	1						
26. <i>Datura Stramonium</i>	100		30		26	
27. <i>Gilia achillæfolia</i>	200	1						
28. <i>Lasthenia californica</i>	200		4		14	
29. <i>Ligusticum Levisticum</i>	100	2		40			
30. <i>Pæonia</i> , mixed vars.	100							
31. <i>Verbascum Thapsus</i>	500							
1842.								
32. <i>Melilotus macrorhiza</i>	250	128	52	16	14	
1846.								
33. <i>Anemone coronaria</i>	100							
34. <i>Arnopogon Dalechampii</i> ...	50	7	5	10	14	
35. <i>Betonica hirsuta</i>	100							
36. <i>Bunias orientalis</i>	50	33	24	20	26	
37. <i>Foeniculum dulce</i>	100	41	43	20	26	
38. <i>Psoralea bituminosa</i>	50	17	29	20	26	
39. <i>Ranunculus caucasicus</i>	100							
40. <i>Rhagadiolus stellatus</i>	50	3	31	16	26	
41. <i>Thalictrum minus</i>	100							
42. <i>Veronica peregrina</i>	100							
1847.								
43. <i>Chenopodium Quinoa</i>	200	46	125	18	14	
44. <i>Panicum Meliaceum</i>	200	62	116	12	14	
1848.								
45. <i>Thalictrum minus</i>	200	29	28	20	14	

Report concerning the Observatory of the British Association at Kew, from Aug. 9, 1848 to Sept. 12, 1849. By FRANCIS RONALDS, F.R.S., Honorary Superintendent.

NOTWITHSTANDING the resolution which was adopted at the last meeting of the British Association for discontinuing observations at Kew (a resolution partly founded upon an opinion that the establishment could not be carried on in a manner satisfactory to the Association "on so low a scale of expenditure" as that which had hitherto been found practicable), it was deemed expedient to furnish a fund for defraying the cost of prosecuting experiments then in progress, together with a few necessary expenses of the establishment; and another sum for the reduction and discussion of the series of electric observations which commenced in August 1843 and terminated in August 1848.

The last year's work has therefore been principally devoted to reduction and discussion by Mr. Birt of the electric observations recorded in the five annual volumes preceding this year's volume, and to the due prosecution of the magnetic experiments which were contemplated.

I much regret that it has not been in my power to do more, as regards the discussion of the observations, than confer with Mr. Birt upon the course which was to be adopted*. The other objects became latterly so pressing, and in my humble opinion so important, that it has been quite out of my power to devote the time and attention to the subject which it eminently deserves. The observations have furnished means of computing results, which, combined with the successful prosecution of experiments on a general photographic system of registration, will, I trust, be deemed ample justification of the opinions expressed by the last Kew Committee, presided over by Sir John Herschel, and participated in by the Council, as to the utility of the Kew establishment†.

Mr. Birt's reductions, &c. will appear in a separate Report; and I now proceed to devote a few lines (as usual), first to the state of affairs at Kew, so far as regards the Building, Instruments, &c., and secondly, to an account of the experiments and operations which have been conducted here during the last (Association) year; and a few other matters connected with experimental inquiry.

I. THE BUILDING, &c.

The premises having been repaired (outwardly) at the expense of Her Majesty's Government in the previous year, nothing additional has been required to be done in that respect; but I am sorry to add that some parts of the interior are sadly afflicted with dry-rot.

The Quadrant Room has, in consequence of the extraordinary solidity of the foundation, contributed largely to the success of experiments on, and to testing the efficiency of, the self-registering magnetic apparatus, which has been sent by the Superintendent of Magnetic Colonial Observatories, our excellent General Secretary, to Toronto. For the immediate support of that apparatus, two solid stone slabs were attached at the base of the wall temporarily.

The principal Electric Conductor has maintained its original vertical position (with the exception of a slight bending towards the north-east, owing to the prevalence of south-west winds) amid the attacks of six years' tempests; and the insulating power of its only support is improved, rather than otherwise, since its erection by *constant heat and age*. A little is

* It was agreed that the Greenwich methods of reducing meteorological observations should (so far as was consistent with the different circumstances) be adopted, with modifications.

† Vide Report of 18th Meeting, p. xvii.

required to be done to prevent the entrance of rain (when violent gusts occur) at the cap.

The Voltaic Electrometers are a little deteriorated (in appearance principally).

The Henley Electrometer (*apud Volta*) is certainly in a rather less efficient state than when new. Friction of *pivots* is ever bad in electrometers, and want of employment increases the evil tendency.

The Wind Vane has been nearly destroyed by a fall, in consequence of some bad soldering at its supporting ring.

The following instruments are all in an efficient state for use in observation :—

The Galvanometer.

Discharger.

Gold-leaf Electroscopes.

Distinguisher.

Three Night registering Electrometers.

Barometers (two).

Thermometer (standard).

Wet-bulb Hygrometer.

Daniel's Hygrometer.

Saussure's Hygrometer.

Balance Anemometer.

Rain and Vapour-Gauge.

The numerous instruments which have been employed in electric, magnetic, and other experiments and extraordinary observations are not materially, if at all deteriorated. They will be carefully enumerated in a general catalogue of the actual contents of the Kew Observatory arranged under six heads, viz.—

1. Fixtures, furniture, &c. found in the building on the 1st of August 1843.
2. Apparatus supplied by means of a subscription in 1843.
3. Apparatus, and materials for apparatus, purchased out of sums granted annually by the British Association, including a 50*l.* grant from the British Association for experiments.
4. Apparatus presented to the British Association.
5. Books the property of the British Association.
6. Articles which are on loan to the British Association.

II. EXPERIMENTS, &c.

Soon after the meeting of the British Association at Swansea in August 1848, being very anxious to proceed with the magneto-registering system, I began to make drawings of apparatus on the plan of suspending the declination magnet at right angles (horizontally) with the index arm (all else remaining as before), in order to procure a greater extent of scale with the same amount of light; but reflecting upon some valuable conversation which I had the honour to hold with Dr. Lloyd at the Swansea meeting, and on some suggestions of his afterwards, I made diagrams and calculations for trying his methods of attaching the lens to the magnet, and deflecting it by separate magnets, or by reversion of it, in order to procure a larger range of the instrument itself. I submitted these ideas, &c. to Colonel Sabine, and received obliging and very useful hints from him. I also consulted profitably Mr. Ross the optician.

At the beginning of November I had made arrangements, drawings, &c. for mounting a magnet on Dr. Lloyd's plan, which it was intended should be tried at Woolwich; but the apartment (or observatory) selected not having ultimately been deemed very well-fitted for the purpose, I thought that the Kew building and the Kew establishment could and ought to be appropriated to the attainment of so desirable an end, that it was one exactly calculated for the proper business of the establishment, and Colonel Sabine agreed in these views I believe.

My continued instructive correspondence with Dr. Lloyd on the subject was very profitable, and new arrangements were in consequence contemplated which were applicable to either plan (viz. that of using a *detached* lens as heretofore, or an *attached* lens with deflectors, &c.), for comparing them at Kew; and Mr. Ross received some final instructions as to the work to be executed.

But in the course of Mr. Ross's operations in December, a considerable improvement occurred to me in the management of the light, viz. that of suppressing the condensing lenses at the object-end of the camera, bringing the index much nearer to the lamp, and employing the focus lenses to procure not only a distinct image of the index, but also a brilliant pencil of light (broad enough for *our* purposes) immediately from the flame itself. By these means the time required to produce the desired effect upon the paper was very considerably reduced. Mr. Malone assisted me in these experiments zealously.

Several improvements were also made in the construction and disposition of the time-piece (*vide* Plate II. K), &c.; and at about this time, after many vain attempts, an improvement in the brilliancy of the flame itself was effected by a modification of Count Rumford's "Polyflame Lamp," of three flat wicks, &c., and an especial adaptation of a *high* square copper chimney, &c. (*vide* Plates II. and III. D).

The reason for not instituting the above-mentioned comparison of lenses, was chiefly that of finding the expense of a lens properly adapted to the purpose very considerable. Yet I trust that my anxiety to carry out practically Dr. Lloyd's important suggestions, combined with the occurrence of more favourable circumstances, may not be ultimately unavailing, or that some less costly method than we thought of may be propounded.

In February 1849 Colonel Sabine wished to know the difference of effect (under such circumstances as those in which the Toronto horizontal force magnetometer finds itself in magnetic storms, &c.) between a *slit* in a shield and an *index*. The slit had also occurred to Dr. Lloyd and others, and I resolved upon attempting a strictly practical solution of the question.

But before the experiment had been tried upon *paper*, it struck me that the Daguerreotype process would be far preferable to Talbotype in these cases of rapid and great variations, if not in *every* case, in consequence of the greater sensitiveness, greater capability of retaining the integrity of its normal condition, and greater delicacy (or sharpness) of outline; and the result of the first trials fully confirmed the Colonel's sagacious anticipations of the superiority of the slit, at the same time that the use of silver surfaces became at once indispensable for future operations. On the 23rd of February, two specimens, extremely well defined, were procured, one in twenty seconds, the other in thirty. The first was the stronger (too strong).

The next problem was to *copy* these impressions, for it was deemed too expensive and cumbersome to preserve them; and I spent much time in trials on Mr. Edwards' plan, viz. that of pressing off a part of the mercury upon black paper coated by a solution of isinglass. The sticking, and consequent tearing of the long piece of paper presented great obstacles (amongst others) to the success of these attempts; and I began, with Mr. Malone's obliging assistance, to try whether the Talbotype process could be applied profitably to copy these metalline impressions. A specimen is preserved; but we arrived at the conclusion that the trouble and cost of *time*, &c. in the execution would be too great, and that no copy on paper could ever be so sharp and beautiful as the metalline impression itself.

In the beginning of April I made a little experimental addition to the

clock-work, for *imitating* long excursions of a magnet in short intervals, in order to prove the efficacy of the above-mentioned new arrangements relative to light, the slit and the Daguerreotype process in such cases, and adapted it to some horizontal-force apparatus which was intended for the Toronto Observatory; for it was by far too tedious a task to wait for any disturbance approximating in extent to those which occur in Canada. A specimen is preserved of the result, which makes out the case (of success) very well. But we already begin to contemn these *dirty*, although efficient, specimens.

I also began to think about etching the impressions on the plate itself, and received some valuable information on the subject from Mr. Malone, Mr. Hodgson of Winchfield, and other gentlemen; and I found that the *usual* cost of plates was somewhat too high.

Toward the end of May, Daguerreotype apparatus for cleaning, polishing, coating, &c. silvered plates of the length required for our purposes claimed attention, with special regard to saving of time and labour.

About the same time Dr. Lloyd visited the observatory, and suggested the advantage of procuring a *zero line* upon the plate formed by the action of the same source of light which produced the magnetic curve (as I had from the first procured on paper), instead of depending upon the edge of the plate for reading off ordinates. This hint appeared so judicious, that (although presenting difficulties in contrivance and execution, and thus creating delay in the preparations for shipment of the Toronto bifilar apparatus) I thought it right to try experiments, and attained the object. The method will be easily understood presently.

I had now also hit upon an obvious, but very useful addition to all apparatus calculated to measure ordinates of magnetic and other curves from a given abscissa, within certain but extensive distances. This instrument I call the Scale Board (*vide* Plate IV. figs. 2 and 3), and will describe it below.

The last-executed improvements have been upon the instruments used in cleaning and coating the plates, in which Mr. Nicklin has materially assisted; and in carefully etching, or rather engraving and etching, the plate without using (at first) a "ground," for which I am chiefly indebted to Mr. Wood. The plate which has been thus treated is still capable of receiving more impressions in the camera, although the first impression is deeply engraved, and capable of printing any (usual) number of copies. A printed specimen is preserved (*vide* Plate IV. fig. 4).

About the first week in June I experienced great satisfaction in receiving a visit from Colonel Sabine, to inspect the apparatus (which had been experimented upon, improved and tested at our *Kew* Observatory, under the auspices of the British Association) for a horizontal-force magnetograph, to go to the *Toronto* Observatory. It (excepting the stone pillars) was shipped for Montreal, and addressed to Captain Lefroy, Director of the Magnetic Observatory, Toronto, in about the middle of last August, and may be thus described.

(Similar letters refer to similar parts in all the figures, excepting in figs. 2 and 3, Plate IV.)

The figures of Plates I. II. III. and fig. 1 of Plate IV. are drawn to one-eighth of size. Fig. 2 and 3 of Plate IV., and all those of Plate V., are one-fourth of size. Fig. 4 of Plate IV. is of real size.

V (Plate V. figs. 1 and 2, &c.) is the magnet-box, coated (as usual) inside and out with gold paper, and provided with a short tube (v^1), which descends and opens into A.

A is the camera box. a^1 is a solid brass casting, forming in part one of its ends.

B is a fifteen-inch magnet, belonging to a bifilar magnetometer of Dr. Lloyd's construction. b^2 is its stirrup. b^3 a pair of light brass tubes, connected with b^2 by entering a short tube attached to b^2 , and permitting a horizontal adjustment (of b^3). The counterbalancing ball at one end is also adjustable (for poising b^3 properly).

b^1 (figs. 1, 3, 4, 5) is the *moveable shield*, composed of very light sheet brass, curved and attached to a little tube, which is clamped by a peculiar nut and screw to the end of b^3 . It has a very narrow slit at its lower edge in the centre.

b^4 is the usual copper damper, the upper and lower central portions being formed into curves for the free "*play*" of b^2 and b^3 . b^5 b^5 are its supports.

O is a diaphragm plate, whose aperture is about an inch long horizontally and a quarter of an inch wide. It carries

o^1 , which is the *fixed shield*, similar in form to b^1 , and attached to O by means of a little bolt, washers and nut (o^2). It is capable of adjustments for horizontality, height, &c. At about three-eighths of an inch from its centre is a slit, somewhat larger than the slit in b^1 . This shield stands at a distance of about a tenth of an inch from b^1 , and at about one-fortieth of an inch higher than b^1 .

C is a glass plate admitting light into the camera. It has in front a small brass sliding-shutter.

D (Plates I. and III.) is a lamp constructed on Count Rumford's poly-flame principle of three flat wicks raised and lowered by rack-work.

d^1 is its high squared copper chimney, provided with a glass plate about three-quarters of an inch high placed opposite to the best part of the flame (or flames).

E is the mouth, consisting of two angular pieces (as seen in Plate V. figs. 1 and 2), and of two little plates attached to them, forming the lips and aperture e^1 , which aperture can be diminished or increased at pleasure after relaxing the little nuts of screws which pass through oblong slits cut through a^1 .

A horizontal aperture, of about a quarter of an inch broad, cut through a^1 , admits the light which forms the focus at e^1 of the *moveable* slit (in b^1), and a little vertical aperture in a^1 admits the focus of the fixed slit in o^1 . The magnetic curve and zero line are produced by these foci respectively.

F is the slider case, for receiving the sliding frame.

f^2 is a perfectly true ruler of brass, attached *vertically* to a^1 by means of three screws passing through it, through three thick washers (or little pillars), and through three oblong slits in a^1 , &c. It is capable of adjustments for perpendicularity, &c.

f^3 is a roller spring, attached to a^1 , and acts upon the slider frame side-wise, pressing it gently against the ruler.

f^5 is a pair of similar springs, acting upon the frame in front, and pressing the glass in the frame against the mouth.

G is the lens tube, containing two groups of Ross's achromatic lenses.

g^1 is apparatus (of sliding plates, &c.) for the support and due centring of G, &c.

g^2 is apparatus of studs, pinion, milled head, key, &c. for moving the rod g^3 , which is attached to the stud at g^4 , and serves for the adjustments to focus (of G).

H is the sliding frame suspended in F. h^1 are the spring bars for retaining the plates, either metallic or glass, in their proper places. h^2 are friction rollers. h^3 is a hook with a screw in it, which clamps the gut line, entering a hole in the top of H.

I (Plates I. to IV.) is the pulley on the hour-arbour of the time-piece.
 i^1 the gut line suspending H. i^5 is the counterpoise to H.

K is the time-piece, with its weights, pendulum, &c., and a lever with fork, k^2 , for stopping and starting the clock at any given second.

k^1 is the support of K and F.

k^2 are brass tubular braces.

P^N and P^S are stone pillars, whose common centres are in the mean magnetic meridian (about).

P^E and P^W are stone pillars, whose common centres are at right angles to the magnetic meridian (*quasi*).

Q are stone brackets fixed in P^E and P^W for the support of V.

R is a cross slab of stone, resting on P^E and P^W .

r^1 is the cross piece of mahogany (used in Dr. Lloyd's arrangement), secured firmly, with means of adjustments, upon R by bolts and nuts, r^2 .

S is the torsion apparatus (of plate, &c.) (Dr. Lloyd's).

s^3 the suspending wire, passing round the grooved wheel.

s^6 , on the axis of which b^2 rests "by inverted Y^S ."

T the glass tube resting on t^1 , which is a fillet contained in t^2 , which is a neck or brass tube attached to V.

X is a black marble slab, carrying A, k^1 , &c., and supported upon P^N and P^S very firmly, but admitting of a small adjustment (on occasion) about the common axis of the suspending wires, s^5 .

Y (Plate IV. fig. 2) is the silvered plate (in the scale board).

y^1 is the magnetic curve produced by the focus of the slit in the moveable shield (b^1).

y^2 is the zero line produced by the focus of the slit in the fixed shield (o^1).

It will be easily perceived that in the arrangement which has now been described no *hygrometric* expansions and contractions can have sensible effect upon the required result, and I believe that thermometric variations are equally unappreciable.

The scale board (Plate IV. figs. 2 and 3), for measuring off rapidly and correctly ordinates formed by the magnetic or other curve with the zero line, is thus constructed:—

A is a mahogany board.

a^1 , &c. are four screws attaching it to

B, which is another heavier board, and which it is well to clamp upon a sloping desk.

C is a ruler attached to B by a screw at each end, passing easily through an oblong aperture, and allowing a lateral free motion of the ruler upon B. A blank ivory scale is fixed upon C.

c^1 is a milled-headed screw, acting by its shoulder upon a piece which presses C inwards, or against the right-hand edge of Y.

c^2 and c^3 are screws passing through another ruler,

M, &c., fixed immoveably upon B, and acting by their ends upon two little brass sliders which press upon the left-hand edge of Y. This fixed ruler (M) carries a scale of white metal, upon which divisions, representing hours, half-hours, quarters and five minutes, are engraved, a length of one inch representing one hour (for the slider H in the case F is moved by the clock at a rate corresponding with these values). Two spiral springs are contained in B, which cause the two sliders pressing on Y to resume their normal positions when c^2 and c^3 are not employed.

T is the ebony stock of the T square.

t^1 is its blade of white metal, upon which is engraved on one of the fiducial edges divisions representing fiftieths of an inch, and on the other sixtieths,

counting from the zero mark, 0, on each series; and it is affixed to T by a milled screw passing through one of the oblong slits at either end, so that either scale may be used, or a blade much more minutely divided might be substituted.

A good double lens, or *pair* of lenses, may be used upon a stand with this apparatus for reading the scales.

The manner of using this instrument is perhaps sufficiently obvious. The zero of the *ordinate scale* (t^1) is adjusted (if necessary) to that right-hand edge and extremity of the zero line (y^2) which is furthest from the *time scale*, M, transversely (after relaxing the screw near T). The ordinate scale is, secondly, applied to the other extremity of y^2 ; and if the zero point on it should not coincide with y^2 , then the screw c^1 is relaxed, and the appropriate left-hand screw (either c^2 or c^3) is slowly screwed up until exact coincidence occurs. Then c^1 is screwed up again.

Particular information, &c. as to the use of the apparatus sent to Toronto was carefully detailed, and some hints relative to the (seemingly) best modes of operating upon the *long* Daguerreotype plates, &c. were set down for the use of Captain Lefroy, &c.

These details are not requisite here. The former *kind* of information has been already published, *i. e.* when my earlier experiments on registration were made known*; and the latter is comprised in great part (although not in sufficient abundance) in several well-known publications.

Proceeding now with the relation of the other circumstances connected with experimental inquiry at Kew, I may add, that at the visit above mentioned of Colonel Sabine we held some conversation on the subject of constructing a *vertical-force magnetograph*, which had previously occupied our attention, when the Colonel relieved me from a difficulty by hinting that an arm might be erected vertically upon the centre of the magnet, to carry the shield with its slit. By this means the injurious proximity of the lamp to the magnet at night will be entirely avoided.

This apparatus is in an advanced state of preparation for Toronto.

My correspondence with the Rev. Alfred Weld, respecting the establishment of a self-registering electric and magnetic observatory at Stonyhurst, after occupying much time (in making plans, drawings, &c.), has not been as yet followed by the erection of a suitable building at that locality.

In August 1848 I received from the Superintendent of the Great Western Railway Electric Telegraph some further and rather curious notices of the deflections of the needles, &c. at Paddington, Slough and Derby. At Paddington, on the 9th of August, at about 1^h 50^m P.M., during a storm, an explosion occurred in the office like that of a gun fired, and the cross wire was fused. The same thing occurred at Slough at the same time.

The most remarkable effects upon these wires are those which are produced by *fogs*; and I apprehend that experiments relative to them would be interesting, and perhaps profitable.

Amongst several distinguished visitors to the observatory in the past year, Don Manuel Rico, Director of the Madrid Observatory, came to converse on the subject of erecting an electrical apparatus like ours at that building, and gave me a rough plan and description of it.

The site appears to be extremely favourable. Experiments and observations in that latitude would form an important link in a geographical series comprising the observations (now probably going on by means of similar apparatus) at Bombay, and others to be instituted in a very high latitude (as Alten, *c. g.*).

* Vide Phil. Trans., Part 1. for 1847.

I trust that other gentlemen, visitors to Kew, have derived some little pleasure, and even profit, from the results of their inquiries here, and that my limited correspondence on electric and other subjects with several gentlemen of scientific eminence has not been wholly profitless to all parties.

I have usually set down under this head a *little* list of proposals for new experiments, or the continuation of old ones; but the number of such-like propositions has accumulated so much faster than the means and time required for their execution, that the catalogue arrives at an almost despairing magnitude. However it shall follow here, because it will at least serve to show that plenty of work could be done at Kew if we had plentiful means.

1. Experiments to determine various points as to the construction of the declination and horizontal-force magnetograph, and particularly Dr. Lloyd's propositions concerning attached lenses.

2. Idem, as to the vertical-force (balance) magnetograph.

3. Idem, as to the completion of a self-corrective system for the barometrograph.

4. Idem, as to the *best* mode of constructing the thermometrograph.

5. Comparison of long and short magnets, and their effects on the registration compared particularly.

6. Experiments in pursuance of some which were commenced here in 1845 on the important subject of "*frequency*" of *atmospheric electricity*; a subject which has been most unaccountably neglected since the observations of *Beccaria* at Turin in about 1750, and one which seems to me to grow in importance with the growth of our chemical and magnetic information.

7. Experiments in pursuance of some which were made at Kew on insulation, and particularly on the insulation of air charged with a known amount of humidity, and at different temperatures, &c., a matter recommended for examination by *Coulomb*.

8. Experiments in pursuance of the same course, but having especial reference to the measures of atmospheric tensional electricity, as indicated by *Henley's* and other electrometers, used in attempting to estimate properly *high* tensions.

9. Experiments on apparatus for observing shooting stars.

10. Experiments on the best mode of pursuing observations on terrestrial temperature, as recommended by *Professor Forbes*.

11. Experiments on kites at known and *constant* elevations, in pursuance of one made at Kew in the year 1847, with a view to their real utility in meteorology.

12. Experiments on the comparative advantages of plate and cylindrical surfaces in reference to their use in self-registering instruments, the former on *William Nicholson's* construction; and also experiments on a mode of reading off the ordinates on such cylinders.

Report on the Experimental Inquiry conducted at the request of the British Association, on Railway Bar Corrosion.

By ROBERT MALLET, M.R.I.A., Mem. Inst. C.E.

It having been long loosely rumoured that railway bars corrode less when in use, i. e. travelled over, than when out of use, and the only evidence for this being that they appear to do so to the eye, and several vague speculations having been broached by engineers and others to account for the assumed facts, it seemed desirable to ascertain the truth experimentally, and also to determine at the same time the constants of abrasion by the action of the wheels of railway carriages, this latter being in fact a necessary prior question to the research as to corrosion.

A general sketch of the views promulgated on this subject is contained in my Third Report on Corrosion of Iron to the British Association in 1843; a sum of £20 having been placed at my disposal by the British Association at the Manchester Meeting in 1842, for the purpose of these experiments.

The first experiments were directed to the object of ascertaining *the fact* of any difference in the amount of corrosion by air and water, &c. between railway bars in use and out of use, in an exact and unexceptionable manner; and from the great weight of the rails requiring a balance of great strength, this was found by no means an easy matter, as the difference of corrosion in any moderate time might be expected not very greatly to exceed the errors of weighing. The first sets of experiments arranged were on the Dublin and Kingstown Railway, upon that part of the line which lies near Sydney Parade, at the Dublin side of the level crossing there.

The line is quite straight, the brakes are never applied here, and the rails are level. Three sets of six lengths of fifteen feet rails each, were here laid down upon the coming into town or western line.

The direction by compass of the rails at this point is north-west and south-east, and hence these rails were always traversed over in a direction from south-east to north-west.

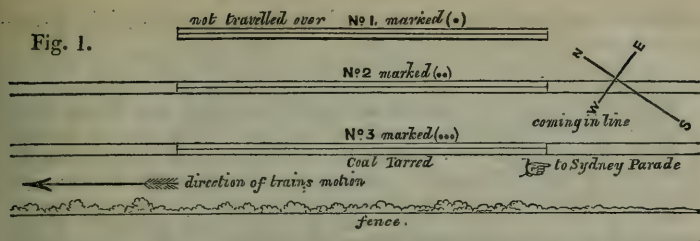
The experimental rails were laid in the following way:—Having been carefully weighed, viz. two sets of bars of six each were laid into the coming-in line, and secured in the same way as all the others on the line, by cast-iron chairs and compressed wood wedges resting upon longitudinal memel sleepers, and with pine or memel filling-pieces between the chairs, filling up the spaces between the bottoms of the rails and the tops of the sleepers.

Of these two sets laid into the line, one set of six bars (No. 2 marked ··) was exposed freely and without any preparation, and was placed on the eastern side of the coming-in line; the other of the two sets (No. 3 marked ···) was coated all over with boiled coal-tar, laid on the iron when hot, so as to protect it from all corrosion. The third set of (6) bars (marked No. 1 ·) was laid upon wood sleepers, chairs, &c., in the same way as the others, but were placed aside by themselves in the middle of the road between the two lines of railway, without any preparation, and freely exposed to corrosion, but not travelled over.

All three sets of bars before being laid down or coated were heated in a boiler-maker's oven to a bright red heat, to remove all rust by a scale, leave their surfaces perfectly uniform and alike, and were permitted to cool slowly without any blows, and in a horizontal position, so as to have as little permanent magnetism as possible.

Thus arranged the three sets of bars stood upon the line in the following order:

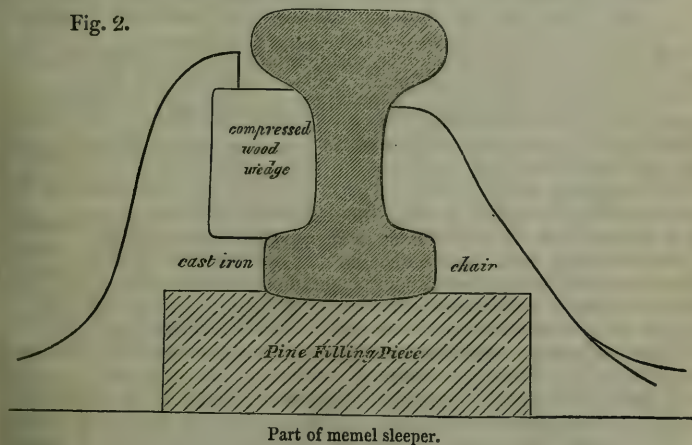
Fig. 1.



Thus the set No. 3, tarred over, is exposed to abrasion alone. The set No. 2 is exposed both to corrosion and abrasion; deducting therefore the amount of the former from the latter, we get the amount of *corrosion alone* of the rails in use, and are enabled to compare this with the amount of corrosion of the set of rails No. 1 out of use, and thus at once to ascertain the difference, if any exists, and to determine the amount of abrasion for a given weight of traffic, of which returns are kept by the Dublin and Kingstown Railway Company.

Half-sized section of Dublin and Kingstown Rail, taken accurately from a filed sheet-iron template of those experimented on, Sept. 1844.

Fig. 2.



The Dublin and Kingstown rails were made at the works of John Bradley and Co. of Stourbridge, and profess to be according to the specification of the Company's Engineer of 1833, viz. "Exterior surface of best rolled iron previously hammered, and the interior of the best puddled iron previously hammered, the proportion being two parts of the former and five parts of the latter, and branded as in margin."

The dimensions as to surface, &c. of the rails is as follows, in accordance with the foregoing section:—

Total perimeter of the rail = 11.5 inches:—

Total surface per yard running = $36 \times 11.5 = 414$ sq. ins. = 2.88 sq. ft.

Total surface in contact with the wheels $= 1.5 \text{ in.} \times 36 = 54 \text{ sq. in.}$ per lineal yard of rail.

Total surface in contact with *filling pieces* beneath, and hence partially protected from corrosion, is $= 1.7 \text{ in.} \times 36 = 61.2 \text{ sq. in.}$ per lineal yard.

Besides this there is the surface covered by the chairs and wedges, which partially but *very slightly* prevent corrosion, water finding its way between in wet weather very readily. There is one chair at every three feet, which covers about twenty-four square inches, including the wood wedge. The meeting-chairs at every fifteen feet cover twice this.

The top surfaces of the rails which are run upon appear to corrode scarcely at all, owing to the fine polish preserved by the rolling of the wheels; if it be assumed that neither this top surface corrodes nor the bottom surface covered with the filling slip, then

The total surface per yard lineal exposed to corrosion is $= 414 \text{ sq. ins.} - (54 + 61.2) = 298.8 \text{ sq. ins.}$, and the uncorroded surface $= (54 + 61.2) = 115.2 \text{ sq. ins.} = 2.08 \text{ sq. ft.}$, or the corroded is to the non-corroded surface per yard, as $298.8 : 115.2$, or as $2.59 : 1$;

omitting any account of the surfaces covered by chairs, which are common to all the three sets, and do not prevent the corrosion materially beneath.

But if the top or running surface of the rail be supposed to corrode equally with the sides, then

The total surface per yard exposed to corrosion is $= 414 \text{ sq. ins.} - 61.2 = 352.8 \text{ sq. ins.}$, and the corroded is to the uncorroded surface per yard as $352.8 : 61.2$, or as $5.76 : 1$.

The set No. 1, not travelled over, corrodes on the top as well as sides, and hence exposes to corrosion per lineal yard, $414 - 61.2 = 352.8 \text{ sq. ins.}$

The weighings of the three sets of railway bars took place for the first experiment on 24th of March 1841: they were previously marked, as mentioned above, (\cdot), ($\cdot\cdot$), ($\cdot\cdot\cdot$), with a centre punch near one end on the side of the bar, and each bar of each set numbered from 1 to 6.

The weighings of the coal-tarred set were of course made before the application of the varnish, in applying which care was taken not to heat the bars so as to *scale* them or to abrade, or in any way alter their weights.

The weighings were made under my own eye, by David James, an intelligent workman, with a beam about six feet long, sensible when loaded with one rail to about 2 ounces avoirdupois, or $\frac{1}{1800}$ th of the load in one scale, or $\frac{1}{3600}$ th of the whole load. Four accurately adjusted half-hundred weights of cast-iron, varnished over, prepared from the brass standard were used, and retained for use again on subsequent removal of the rails, and the other weights were accurate brass standard avoirdupois weights of my laboratory.

Each rail was weighed separately, and the weights were checked by myself.

The following table gives the data and numerical results of the first series of experiments.

All three sets, No. 1, No. 2 and No. 3, were laid down, and the traffic of the second day of July 1841 was the first that went over them.

They were all taken up again and reweighed on the 30th of April 1842, being exposed to corrosion and traffic for an interval of 303 days.

TABLE No. 1.—First set of Experiments, Dublin and Kingstown Railway : rails traversed from July 2, 1841 to April 30, 1842 only : period 303 days.

Number.	Mark of the bars.	How exposed, &c.	Total weight of each rail when exposed. July 2, 1841.	Total weight of each rail when removed. April 30, 1842.	Gross weight of the set when laid down.	Gross weight of the set when removed.	Total loss of weight in each set.	First difference = corrosion in use.	Second dif. = diff. between cor. in and out of use.
1	:		cwt. grs. 2+15,385	cwt. grs. 2+13,125	grs.	grs.	grs.	grs.	grs.
2	:	Set No. 1.	2+15,175	2+13,125					
3	...	In middle of line, and <i>not</i> travelled over.	2+ 1,845	2- 1,750					
4		2+14,875	2+13,125					
5	Corrosion alone.	2+ 3,900	2+ 1,750					
6		2+32,375	2+28,875	9,491,555	9,476,250	15,305...	
1	:		2+12,250	2+ 6,890					9043
2	:	Set No. 2.	2+ 7,885	2+ 1,970					
3	...	On the East side of Up Line.	2+17,500	2+12,142					
4		2+13,410	2+ 7,875					
5	Corrosion and abrasion.	2+29,975	2+21,875					
6		2+41,750	2+40,250	9,530,770	9,499,002	31,768		
1	:		2+ 7,356	2+ 3,425					6262
2	:	Set No. 3.	2+23,070	2+18,775					
3	...	On the West side of Up Line.	2+ 3,750	2- 1,275					
4		2+ 2,950	2- 875					
5	Coal-tarred.	2+21,830	2+16,550					
6	Abrasion only.	2+11,900	2+ 8,750	9,478,856	9,453,350	25,506		

We are enabled to draw the following conclusions from this Table No. 1:—

1st. On thirty lineal yards of rail in use, the amount of abrasion is=25,506 grs. in 303 days=30,725 grs. per annum, or at the rate of $\frac{30725}{30} = 1024.16$ grs. per yard per annum.

The following is Mr. Bergin's statement of the amount of traffic which passed over the line from 2nd July 1841 to the 30th April 1842.

Passengers.—1841.

July	67,172
August	98,540
September	67,901
October	52,234
November	39,355
December	36,809
Total, exclusive of Subscribers	362,011
Add Subscribers	91,745

1842.

January	37,674
February	35,534
March	42,509
April	53,211
Total, exclusive of Subscribers	168,928
Add Subscribers	23,226
Total of passengers in 303 days	645,910 persons—
which, divided by 15, is =	43,061 tons.

Engines and Carriages.

No. of trains in all=10,528=same number of engines and tenders
at 12 tons each =126,336 tons.

No. of 1st class carriages=10,528

No. of 2nd class carriages=33,427

No. of 3rd class carriages=30,364

Total of carriages=74,319 at $3\frac{1}{4}$ tons each=141,537 tons.

Gross weight of engines and carriages.... 267,873 tons.

Hence the total gross traffic in carriages and passengers
in 303 days is =310,934 tons.

But a quantity of luggage and parcels are carried on
the line of which no correct account is kept; assuming
this at an average of 10lbs. for each passenger, which
will probably be about the truth, we have $\frac{645910 \times 10}{2240} = 2,883$ tons.

303 days' total traffic..... 313,817 tons.
or 378,030.38 tons per annum.

But as the load is uniformly diffused over both sides of each set of rails,
only one-half the above load passes over any one given length of rail—

$$\text{or } \frac{378030}{2} = 189,015 \text{ tons,}$$

the passage of which produces an abrasion of 1024.16 grs. per yard per
annum. Hence $\frac{1024.16}{189015} = 0.00542$ grs. per ton per yard,
or $1760 \times 0.00542 = 9.5392$ grs. per ton per mile.

There were 10,528 trains passed over the rail in 303 days; assuming these
uniformly diffused over the 24 hours, it is $\frac{10528}{303 \times 24} = 1.447$ train per hour;

but as the trains only travel from 6 o'clock A.M. to 10 o'clock P.M., or 16
out of the 24, it is at the rate of 2.171 trains per hour, or rather more than
one every half-hour. This is probably as fast as locomotive trains are likely
to travel constantly on any line; but the *actual weight* of each train will
materially affect the amount of abrasion, as there can be no doubt that at
some certain weight the substance of the iron would be *ruffled* and disinte-
grated by the great pressure rolling over it.

We can determine in this case the average weight per train, as follows:
viz. 43,061 tons of passengers + 2883 tons of luggage

$$= \frac{45944}{10528 \text{ trains,}} = 4.36 \text{ tons of passengers and luggage per train,}$$

$$\text{and } \frac{267873}{10528 \text{ trains.}} \text{ tons of engines and carriages} = 25.44 \text{ tons average per train.}$$

Hence the average gross weight of each train is $=29.8$ tons, or nearly 30 tons.

And the remarkable fact appears, that the *useless* load per train is to the *useful* as $25.44 : 4.36$, or as $5.83 : 1$, or nearly as $6 : 1$; and that the abrasion or destruction of rail relatively to the useless and useful load are in the same ratio.

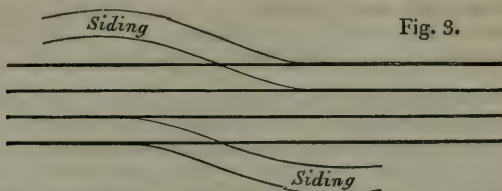
2nd. We deduce from this, that the absolute corrosion of a length of rail out of use to that of the same rail in use, or exposed to traffic, is in round numbers about, as 15.30 to 6.26 , or that the difference in favour of the latter is 9.04 ; but it will be best to postpone a minute comparison of the rate of corrosion until we have the results of the further experiments also before us.

While these experiments were in progress, it seemed very desirable to me to obtain a set of experiments made co-ordinately with the above, but upon a single line of railway where the traffic would be in both directions, viz. backward and forward over the same set of rails; as from views suggested by Mr. Nasmyth, it was possible this might be an important element in the question. Mr. Nasmyth's views, which are briefly alluded to in my Third Report on the Corrosion of Iron, will be found more particularly detailed by himself in the following interesting letter to me, which I have his permission to publish:—

" Bridgewater Foundry, Patricroft,
May 19, 1842.

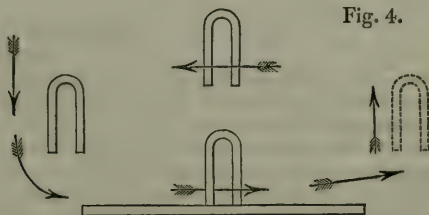
"MY DEAR SIR,—On my return from the continent I had the pleasure to receive your valued letter respecting the rusting and non-rusting of railway rails. I have not had any opportunity to obtain the exact comparative rate of oxidation under the two conditions, but so strikingly different is the oxidation of the one, as compared with the other, that a very slight glance will satisfy any one that they are under very marked and different influences, inasmuch as, in the case of the Liverpool and Manchester, the rails of which have been laid and exposed to all the changes of wet and dry for upwards of five years, there is no more appearance of rust than merely a light-brown coating of mud-coloured water, more the result of the splashing of rain; while in the case of the London and Blackwall Railway, in which the carriages travel alternately east and west on *the same* rail, the rusting is proceeding at that rate, that although they have not been laid two years, *cakes of rust* are falling from the sides of the rail, and the ground for 12 inches on each side of the rail is yellow with rust. This may be said to be *mere ocular* demonstration, but to any one willing to be convinced, it is most satisfactory proof. I should be most glad to have my observations and theory on the subject brought to the most severe test; but to do this would not be very easy, as to time and similar circumstances, all but the one in question, viz. the one way travelling *versus* the both ways alternately; for to be a true experiment, we should have both bars of the same iron in the same place, only one travelled on in *one* direction, the other in both, and an equal amount of travel on each. The experiment required in that form might be tried by mechanical contrivance, *but* then we know not as yet what is due to the correction with so vast a length of rail as in the case of railroad; but in the absence of any very delicate and 'scientific-like' results I am fain to content myself with the most striking difference, which is observed, or may be observed, by any one whose attention is directed to the subject. I may also mention, that on the Liverpool and Manchester line, all the sidings, as they are called, *i.e.* those parts of the rail which serve for backing trains into when it is desired to permit others to pass them on the same line,—that all

such sidings are rusting most rapidly; it is the *sides* of the rail I hold to as proof, as such sides are in both cases removed from any friction. I may also



name that even the *keys and chairs* partake of the rusting or non-rusting influence, as the case may be.

"I have had no means as yet to ascertain whether my conjecture is right or otherwise; but I consider the rolling of the wheels in *one direction* to confer or induce a magnetical condition on the rails, in the same manner as in the case of inducing magnetism or magnetical property on a piece of iron or steel, by the ordinary method of passing the parent magnet along the iron bar, thus:



The subject is, I think, a very interesting one, and well-worthy of attention, as it may tend to illustrate, on a most grand scale, some of the pure results of the delicate investigations which I doubt not you are familiar with, both as to what has been brought to light by others as well as yourself. If there be any further questions I can answer, you may command me at all times, as

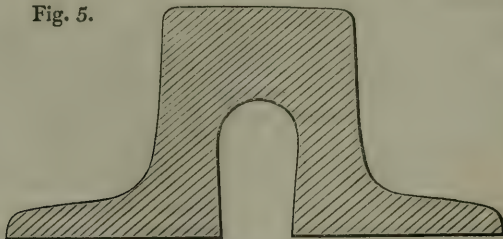
"I am most sincerely yours,

"Robert Mallet, Esq."

"JAMES NASMYTH."

The Ulster Railway between Belfast and Portadown, which was at that time a single line on the wide gauge, with a bridge rail of 52lbs. per lineal yard as per fig. 5, half-sized section given, and without any chairs, and resting

Fig. 5.



on longitudinal wood sleepers, presented an excellent position for this expe-

periment, and on my writing to Mr. John Godwin, C.E., the Engineer of the line, he at once acceded to my wishes, and undertook the experiment. The following table gives its results, which are not as satisfactory as could be desired, owing to some circumstances which are unexplained, and which induced Mr. Godwin himself to consider the experiments in that light.

The two following letters relate to this, and show that care appears to have been bestowed on each step of the process. The only error I am able to remark is, that one-half of the rails B, intended to be exposed to corrosion only, were by some mistake coated to prevent corrosion; hence in deducing the results of the experiments I have been obliged to double the loss on the three uncoated rails B, so as to get an approximation to the truth.

"Belfast, 8th September 1843.

"DEAR SIR,—I send you enclosed the result of the experiment on the rails which we laid down in June 1842. You will observe that they were taken up in June last, and I would then have sent you the particulars had they not appeared so *unsatisfactory*. I cannot account for the great difference in the loss of weight, for we were very careful in weighing them. The quality of the iron could scarcely have made the difference; however, I send you the particulars, and you can draw your own conclusions.

"B, 4, 5, 6, were coated, and of course lost nothing.

"Robert Mallet, Esq."

"I am, dear Sir, sincerely yours,

"JOHN GODWIN."

"Belfast, 14th November 1844.

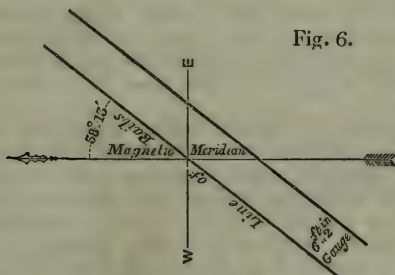
"DEAR SIR,—In reply to your questions relative to the account of the experiments on the corrosion of iron rails which I furnished you with, I beg to say, that the rail *not* travelled on was the centre rail in the middle of the line; they were weighed with the same beam and weights as when put down; the weights were properly adjusted; the beam was sensible to a quarter of an ounce; the rails were weighed separately.

"Robert Mallet, Esq."

"I am very sincerely yours,

"JOHN GODWIN."

Fig. 6.



The direction in azimuth, in which the experimental rails were laid upon the Ulster Railway, was north-east and south-west, $38^{\circ} 13'$ east of north by compass, as in diagram above.

TABLE B. deduced from Mr. Godwin's Experiments on the Ulster Line, laid down June 15, 1842, removed June 27, 1843: period 377 days.

Number.	Mark.	How exposed.	Total weight when exposed of each rail.				Total weight when removed of each rail.	Gross weight when laid down.	Gross weight when removed.	Total loss of weight in each set.	First difference = corrosion in use.	Second dif. = diff. between cor. in use and out of use.
1	⊙ B.	Set No. 1. Between the rails ⊙ not travelled over. Corrosion alone.	12 cwt.	qrs.	lbs.	oz.	12 cwt.	qrs.	lbs.	oz.	grs.	grs.
2			3	22	3	15	3	21	15	One-half, viz. 4, 5, and 6 rails were coated by mistake; hence we must double the loss to get an approximate result. 4lbs. 3oz. = 29,312	}
3			2	3	19	8	2	3	19			
4			2	3	22	4	2	3	21			
5			2	3	27	3	2	3	27			
6			2	3	19	3	2	3	19			
			2	3	19	9	2	3	19	1	17.2.17.15½	17.2.15.14
1	⊙⊙ C.	Set No. 2. Exposed to traffic and corrosion. Uncoated.	2	2	18	15	2	2	17	12	11lbs. = 77,000	}
2			2	3	12	15½	2	3	11	6		
3			2	3	27	11	2	3	25	0		
4			2	3	1	10½	2	3	0	6		
5			2	3	17	15	2	3	15	12		
6			2	3	22	7	2	3	20	6		
										17.0.17.10	17.0. 6.10	
1	⊙⊙⊙ A.	Set No. 3. Exposed to traffic abrasion only. Coated.	2	3	20	7	2	3	19	2	4lbs. 1½oz. = 28,656	}
2			2	3	22	9½	2	3	21	5		
3			2	3	19	9	2	3	19	7		
4			2	3	20	5	2	3	20	2		
5			2	3	25	5½	2	3	25	0		
6			2	3	21	5½	2	3	20	8		
										17.2.17. 9½	17.2.13. 8	

On examining this Table B, it appears that—

1. The absolute loss from abrasion only is = 28,656 grs.
2. The absolute loss from corrosion only is = 29,312 grs. on the rails not travelled over; and
3. The absolute loss from corrosion only is = 48,344 grs. on the rails exposed to wear of traffic.

Hence in this case the corrosion of the rails out of use is less than that of the rails in use in the ratio, in round numbers, of 29·3 to 48·3, contrary to the received notion.

As doubt rested on these results, owing to the circumstances already detailed, I determined to lay down a fresh set of prepared rails upon the Kingstown Railway, and subsequently another set upon the Dalkey Atmospheric Line, which, being a single line, stood in the same predicament as the Ulster Railway.

One of the greatest difficulties attending experiments of this character, consists in the extremely small amount of weight to be determined (namely, the small loss by corrosion, even in a prolonged period), compared with the weight of the rails themselves, and the great absolute weight of the latter demanding balances of great strength, which are very difficult to be given the requisite sensibility. Where balances only competent to weigh one length of rail at a time are used, as in the case of Experiment No. 1, then the several sources of inaccuracy in each operation of weighing are multiplied by the number of rails weighed. As, therefore, the error of large balances does not increase quite as fast as the size of the instrument is magnified, it appeared advisable to obtain means of weighing several lengths of rails at once or together; and for this purpose new standard weights were required, as well as new balances.

The standard, namely, a brass authorized copy of the standard 56lbs weight, in the custody of the Corporation of Dublin, which is understood to be a true duplicate of that formerly in the Exchequer Office,

London, was obtained prior to Experiment No. 1, and was now again used to adjust fourteen cast-iron 56lbs. weights by, so as to enable 7 cwt. of rails to be weighed at once. These cast-iron weights, prior to final adjustment, were coated with a thin covering of copal varnish, to preserve them from corrosion, until again called into use, after the lapse of two years. They were handled with leather slings to avoid abrasion, and preserved in a perfectly dry place, and checked against the brass standard again before being used to weigh the rails after their removal. A steel beam of 36 inches in length was prepared and carefully adjusted by Mr. Yeates, instrument-maker, Dublin, by which these weights were adjusted. It was sensible to 20 grs., with 56lbs. avoirdupois in each pan, or to $\frac{1}{39200}$ th part of the whole load. It turned in all its bearings on hardened steel edges, and was found a most satisfactory and accurate instrument.

A large beam was also prepared and carefully adjusted, whose length was eight feet, and whose strength was such as to be capable of weighing three lengths of rails at once, or of sustaining a load of above 12 cwt. This beam was of wrought iron, turning on hardened steel knife-edges, and with means of gradually bringing the load upon the beam without jar or vibration.

When loaded with three rails and their counterpoise, this very large beam was found to be sensible to 500 grs., or to $\frac{1}{9462}$ d part of the gross load; and could have been made still more so if requisite. It is probably the largest and most accurate beam ever made.

Both this and the smaller beam were tolerably equibrachial; but to avoid any error from this source, double weighings were made in adjusting the weights, and one end of the large beam was marked, and the rails always placed under it, the counterpoise being at the opposite end.

This may appear a tedious description of an unnecessary amount of care; but when it is recollected that the question to be determined relates to a weight of not much more than a single pound in a gross load of nearly 1400lbs., it will be seen that any inaccuracy in the weighings would materially modify, or wholly vitiate the results; and it is to the accumulation of slight errors of this sort, and probably more particularly to want of equibrachiality in the beams used, or want of attention to always weighing at the one end, that I attribute the want of consistency of the results obtained on the Ulster line of railway.

In order also further to increase the accuracy of the result, I proposed to allow a longer period to elapse before again removing the rails from the line when laid down.

The same set of eighteen rails divided into three classes of six each, which had been used in the first experiment on the Kingstown line, were now again brought into requisition. They had lain since the former experiment horizontally under cover in a dry place, and had acquired a very slight coat of red rust. They were all placed in a boiler-maker's oven, and exposed to a bright red heat, and then permitted to cool, without being exposed to any blows or jars, in a horizontal position and under cover. They had all now an uniform coat of black oxide (Hammerschlag), very thin and adherent, were pretty free from magnetism, except that due to terrestrial induction; and in this state they were all weighed, and the weights registered, each rail having received a permanent mark at one end.

The six rails to be exposed to abrasion only, were now heated horizontally to about 400° Fahr. and coated with boiled coal-tar, which rapidly dried into a tough japan varnish.

The weighings were made on the 10th October 1842; and on the 18th October 1842 they were placed upon the up line of the Dublin and Kingstown

Railway, at the same place, and in precisely the same order as before, and travelled over for the first time on that date.

They remained exposed to traffic and corrosion for two years, and on the 18th October 1844 were removed from the line and brought home for examination and weighing. Prior to this the beams and the standard weights were again examined as to their accuracy and adjustment, which were found as perfect as before. I prepared to weigh the rails in the same order as before.

These rails having been divided into three classes or sets, viz.—

No. 1. Not coated, exposed to corrosion alone and not to traffic.

No. 2. Not coated, exposed to corrosion, and also to the abrasion of traffic.

No. 3. Coated with coal-tar and exposed to abrasion of traffic, but protected from corrosion,—

presented, when removed, the following appearances:—

The set No. 1 had a very dark red rusty colour, and an obvious scale of adherent rust all over, which a closer inspection, and on passing the point of the finger over the surface, proved to be *papular* or tubercular, and nearly uniformly so all over, each separate circular tubercle of oxide being about $\frac{1}{10}$ th of an inch in diameter. The spaces between these were less dark-coloured, or buffish; this aspect was quite uniform over every part of the rails, except where they had been in contact with the chairs.

The set No. 2 had no scale of rust on the surface, but a perfectly uniform dark buff or reddish buff thin dusty coating of oxide all over the sides and edges; the top surface was bright and polished by traffic, but the wear was not perceptible in dimension; the lower surface, where in contact with the wood filling slips, on the sleepers was of a deeper colour, and where in contact with some parts of the chairs was bright and polished from the effects of jarring or vibration produced by traffic. There was no loose rust whatever on any part.

The set No. 3, which had been coated with coal-tar, were found bright and polished, like No. 2, on the top edge, where borne upon by traffic. The coal-tar varnish was fresh and sound everywhere else, and no rust had taken place, nor any scaling off from any part of the bars. The surface, however, until it was washed clean, presented a uniform tint of yellowish brown, arising from the fine particles of rust from the other rails, and probably also from the wheels, of passing trains being blown upon the coal-tar coating, and washed upon it by rain, &c.

Prior to being cleaned for weighing, the whole of these rails were examined as to their magnetic condition. The results ascertained will, however, be best reserved for a subsequent part of this Report.

The sets of uncoated rails, Nos. 1 and 2, were rubbed briskly with a fine wire brush until all adherent rust was removed, and then finally cleaned with dry cloths.

The set No. 3 was exposed to a heat of about 700° Fahr. over a charcoal fire, until the whole of the coal-tar coating was burnt, and removed as charcoal dust by the brush and cloth. The weighings were then made in the same order and way as before; and the following Table No. 2 gives the results.

The rails, after being cleaned and weighed, presented all over a light reddish black tinge, perfectly uniform, and free from any scaling, or other indication of unequal action.

There is therefore on this second experiment a distinct corroboration of the result of the former Table No. 1, viz. that there is a real diminution of corrosion in the rails, due to traffic. The absolute amount of difference is less however in this second experiment than in the first. By Table No 1 it appears that the ratio of the corrosion of the unused, to that of the used rails, was as 15·30 : 6·06 ; but in the present case we find the ratio to be as 83·12 : 48·13, or as

1 to 2·5 in the former, and
1 to 1·7 in the latter.

Hence the difference is a decreasing one, the causes of which we shall again refer to.

The whole three sets of rails in this experiment were weighed carefully before being cleaned just when removed from the line, and without any adherent rust or other matter being shaken off from them, and, as already stated, again weighed after having been brushed and cleaned. The difference showed the amount of *adherent oxide attached as a scale* to the uncoated rails, and of varnish coating on the others.

The weight of detached matter was as follows:—

No. 1. Uncoated, not travelled over. 5,250 grs.

No. 2. Uncoated, exposed to traffic 1,313 „

No. 3. Coated, and exposed to traffic 11,375 „

consisting all of coal-tar and dust.

From this it is apparent that the coat of adherent rust upon the unused rails was on equal surfaces to that on the rails exposed to traffic, as 52·5 to 13·1, or that the adherent rust on the unused rails is nearly four times as thick as on the rails exposed to traffic, proving that *the oxide formed on the latter is constantly shaken off by the vibration of passing trains.*

It is now desirable to give the amount of traffic which passed over the rails during the period of the last experiment, viz.—

Traffic in tons passed over the Dublin and Kingstown Railway between 18th October 1842 and 18th October 1844, per T. F. Bergin, Esq.

	Tons.
4,041,075 passengers at 15 per ton	269,405
59,243 engines at 15 tons each	888,645
437,791 coaches, average $3\frac{1}{2}$ tons each	1,532,268
Total in both directions	2,690,318

The traffic being precisely equal up and down, and the passengers very nearly so, say for gross traffic over experimental rails one-half the above= 1,345,159

To which add for ballast brought over experimental rails during the two years, and for luggage 10,000

Total load transferred over experimental rails= 1,355,159
or 677,579½ tons per annum. Only half this, however=677,579½ tons in the two years, or 338,789¾ tons per annum, traversed each length of experimental rails.

This latter weight produced in the two years an absolute abrasion on 30 yards of rail of 47,687 grs. avoir., or of 23,844 grs. per annum, which is nearly 795 grs. abraded per yard per annum, or an abrasion of iron amounting to ·00235 gr. per ton per yard, or $1760 \times \cdot00235 = 4\cdot136$ gr. per ton per mile.

The absolute abrasion is therefore less in this second experiment than in the first, in the ratio of 4 to 9·5 in round numbers, proving that the upper surface of the rails gradually alters in texture, and gets hardened by the rolling over it of the loads, so as to be less and less abraded in proportion to the load passed. This fact, however, can only apply to cases where the

loads are light enough not to disintegrate the surface of the rail, and to places where the brakes are not applied. On many of the lines of heavy traffic in these kingdoms at present the incumbent loads seem from the very first to break up the molecular arrangement of the upper flange of the rail, and hence induce a gradual increase instead of decrease of abrasion; while in places where the brakes are habitually applied, the rails are ground away in flakes with great rapidity, those at some of the stations on the Kingstown line having one-half the upper flange of the rail cut away in three or four years.

Through the kindness of Capt. Larcom, R.E., I am enabled to give the amount of rain which fell in the basin of Dublin during the period occupied in this last experiment. The results are taken from the meteorological register kept at the office of the Ordnance Survey, Mountjoy Barrack, Phoenix Park, Dublin. The rain-gauges are situated on a plain 181·8 feet above the Ordnance datum, or low water of spring-tides, at Dublin Bay lighthouse, and have no hills in the immediate vicinity. The annual fall of rain is pretty constant at Dublin; and hence these tables may be viewed as sufficiently applicable to all the experiments related in this report.

The average rain, from several years' registry, is 33·115 inches by Ordnance gauge, and 29·616 inches by that of the Royal College of Surgeons in the city of Dublin, and at an elevation of 51·72 feet above the Ordnance datum.

Months.	1841.	1842.	1843.	1844.
January	1·767	1·147	1·886	1·726
February	1·210	2·860	1·561	2·517
March	1·635	2·314	1·704	2·058
April	1·082	0·996	2·984	1·207
May	2·349	3·673	4·639	0·295
June	2·043	2·256	2·887	1·479
July	2·763	3·183	2·246	2·039
August	2·951	1·580	2·025	3·634
September	1·489	3·451	1·235	2·847
October	4·810	1·734	3·918	2·824
November	2·781	5·234	2·543	4·992
December	3·245	1·126	0·414	2·412
For the year ..	28·125	29·554	28·042	28·030

The mean barometric pressure for the years 1842 and 1843, corrected and reduced to 32° Fahr. at Dublin, was—

1842..... 29·926 inches,

1843..... 29·870 inches,

the cistern of the barometer being 24·5 feet above the Ordnance datum; and the above numbers being deduced from 3600 observations.

For these data I am indebted to Professor Lloyd, who obligingly extracted them from his results obtained at the Magnetic and Meteorological Observatory of Trinity College, Dublin.

The mean pressure at Greenwich, where the barometer is 159 feet above the level of the sea, for the years was—

1841..... 29·687 inches,

1842..... 29·832 inches,

the instruments being strictly comparable.

The relations to the corrosion of iron, of variable quantities of rain and of atmospheric pressure may be referred to in my Third Report on the Corrosion of Iron, Trans. British Association (sects. 286, 305).

The same sets of rails that had been taken up from the Kingstown line in October 1844 were laid by in a dry place, passed through the same ordeal of heating to a bright red, cleansing and coating (one of the sets) as before; and on the 7th January 1845, the weighings being all completed, the three sets were again laid down on the Dalkey atmospheric single line railway, at a straight part of the line near the Dalkey terminus, in quite the same way and order as before, and fastened in the same way, the set of unused rails being laid at one side of the line of way. The direction of the railway at this place is nearly W.N.W. and E.S.E., corrected for variation; and the traffic is here *in both directions* over the same rails.

The rails were continued on this line for the long period of four years; and on the 7th January 1849 were removed and weighed after prior examination as to state of surface and magnetism, &c. The results are given in—

TABLE No. 3.—Third set of Experiments on the Dalkey Atmospheric Railway: rails traversed from W.N.W. to E.S.E. and the reverse: period from January 7, 1845 to January 7, 1849=1460 days.

Number.	Mark of bars.	How exposed.	Weight of rails as weighed together when exposed.		Weight of rails as weighed together when removed and cleaned.		Gross weight of the set when laid down.	Gross weight of the set when removed and cleaned.	Total loss of weight in each set.	First difference = corrosion in use.	Second difference = diff. between cor. in use and out of use.
			cwt.	grs.	cwt.	grs.	grs.	grs.	grs.	grs.	grs.
1	.	Set No. 1.									
2	..	In middle of line, and not travelled over.	6+	8,500	6-	9,250					
3	...										
4										
5	Corrosion alone.	6+	28,625	6+	13,125					
6										
							9,445,125	9,411,875	33,250...	
1	..	Set No. 2.	6-	14,500	6-	58,750					
2	..										
3	..										
4	...	On the East side of Line.									
5	Corrosion and abrasion.	6+	39,375	6						
6										
							9,432,875	9,349,250	83,625		8800
1	...	Set No. 3.	6-	9,625	6-	44,650				24,450	
2	...										
3	...										
4	...	On the West side of Line.				Broken out + 990					
5	Coal-tarred.									
6	Abrasion only.	6-	9,625	6-	35,000	9,388,750	9,329,575	59,175		
6										
						Broken out + 235					

On examining this table we are enabled to deduce the following results, viz.—
 1st. The absolute abrasion during the whole period of 1460 days on the six rails is 59,175 grs. avoird.

2nd. The absolute corrosion on the six rails in use, and exposed to traffic in same time, is 24,450 grs. avoird.

3rd. The ratio of abrasion to corrosion on the rails in use is therefore nearly in the ratio of 59·2 to 24·5, or about as 2·4 to 1.

4th. The absolute corrosion of the six out of use and not travelled over was 33,250 grs. avoird.

5th. The ratio of the abrasion of the rails in use to the corrosion of the rails out of use is therefore nearly as 59·2 to 33·3, or about as 1·44 to 1, or $1\frac{1}{2}$ to 1.

6th. The absolute difference between the corrosion of the rails in use and out of use is 8800 grs. avoird. Hence

7th. The ratio of the corrosion of the rails in use is to that of the rails out of use as 24·5 to 87·9, or nearly 88, or in round numbers as 3·14 to 1.

Here again then we have corroborated the fact of a real difference in corrosion due to traffic, and again we find it a decreasing one as compared with the former experiments.

We now proceed to give the amount of traffic on the Dalkey Atmospheric Railway during the four years of these experiments, as deduced from the records of the Company by Mr. Bergin at my request.

The whole traffic up and down passed over the experimental rails.

Traffic of the Dalkey Atmospheric Railway from 7th January 1845 to 23rd November 1848.

No. of trains.	No. of coaches.	No. of passengers.
86,972	296,048	949,636

The average proportion of the classes of 2nd and 3rd class carriages is one of equality, one of the latter being always a piston carriage; and the average weights are—

Piston carriage	5 tons	1 cwt.
Second class coach	3	11
Third class coach	3	6

And taking the passengers at 14 to the ton to allow for luggage, we have for the weights in the above time—

1,166,155 tons of dead weight in trains
 67,831 tons of passengers.

Total. . . 1,233,986 tons of gross load.

But the line was stopped on the 23rd November 1848 for repair, and worked by a locomotive. The estimated traffic for this period up to January 7, 1849, is thus:—

	Tons.
288 locomotives at 10 tons	= 2880
214 second class coaches	760
214 third class coaches	706
4274 passengers, 14 per ton	305
Total	4651

Which, added to the foregoing, gives for the whole period of traffic—

1,233,986

4,651

1,238,637 tons

divided into 86,972 + 288 = 87,260 trains. The average weight per train is therefore only 14·2 tons.

The total load transferred per annum on the average was 309,659½ tons. The half of this = 154,829¾ tons was therefore transferred over each length of rail annually.

The total abrasion on 30 yards of rail we have noted at 59,170 grs. in four years, or 14,792·5 grs. per annum, which again is equal to 493·08 grs. per yard per annum. This is equivalent to ·00318 gr. of iron abraded per ton per yard, or to $1760 \times \cdot 00318 = 5\cdot597$ gr. per ton per mile, or nearly 5·6 grs. per ton per mile.

This result corresponds closely with that of the second experiment on the Dublin and Kingstown line, from which we may remark, that although the average weight per train in this instance is only about one-half that of the Dublin and Kingstown line, yet that the abrasion is nearly as great, proving that traffic over the same rails in both directions exercises a destructive effect upon the molecular constitution of the iron, which is equal with trains of a given weight to that produced by trains of double the weight always moving in the one direction only, or in other words, *that with equal rolling loads the destruction of the rails by abrasion is doubled by running the traffic in both directions over the same rails.* Owing to the fact that the piston carriage on atmospheric railways has to open the valve, there is rather more pressure exercised by this carriage upon the rails than is due solely to its weight, but this excess is so small as not to affect the question. Hence the excess of abrasion must be due to the motion in opposite directions continually splitting up the topmost fibres of the iron, which have been partially laminated and rolled out by the former train in the contrary direction of motion.

Having now arrived at the last of these prolonged experiments, we may combine the results into one table of the 1st and 2nd experiments on the Kingstown line, and of that on the Dalkey line, rejecting that on the Ulster as dubious, and reducing all the results to one common period of 365 days, or one year.

TABLE No. 4.—Results of 1st, 2nd and 3rd Series of Experiments reduced to a common period of 365 days.

Nature of the action on the rail.	First Experiment on Dublin and Kingst. Railway.	Second Experiment on Dublin and Kingst. Railway.	Third Experiment on Dalkey Railway.
	grs.	grs.	grs.
Abrasion in use	30,725	23,843+	14,794—
Corrosion in use. . . .	7,523	2,406+	6,113—
Corrosion out of use . .	18,436	6,562+	8,312+
Difference between corrosion in use and out of use. . }	10,893	4,156	2,200

The preceding are the absolute losses of weight upon 30 yards of rail in one year.

The following are the first differences respectively between the abrasions and corrosions as given in the 1st, 2nd and 3rd experiments, *i. e.* differences between 1st and 2nd, and between 2nd and 3rd, viz.—

	1st and 2nd.	2nd and 3rd.
Abrasion	6,882	9049
Corrosion in use	5,117	3707+
Corrosion out of use	11,874	1750+

These do not present a series, but we are enabled to conclude from Table 4,—

1st. That the abrasion by traffic on the same rails constantly decreases in reference to the rolling load.

It is probable that the rate of this decrease will be more and more slow, and at a certain point of hardness reached by the condensation of the iron of the rail due to the rolling load, it will become and continue constant.

2nd. The corrosion both in use and out of use appears also to decrease gradually upon the Kingstown line. The absolute corrosion in both cases is greater on the Dalkey line, owing to the increased dampness of the situation in which the rails were necessarily placed for experiment upon it, viz. in a shallow cutting with wet bottom.

3rd. The difference between the corrosion in use and out of use, which exists throughout all the experiments, is also a constantly decreasing one.

For purposes of general comparison, and of comparison with the corrosion of rails made of other makes or qualities of iron than those of the present experiments, it will be convenient to arrange the following table of the amounts of corrosion, both in and out of use, reduced to one square foot of corroded surface, and for a term of one year.

The total exposed surface of the Dublin and Kingstown rail per lineal yard, is=2·88 square feet, but from this we have to deduct the top surface of the rail, which is rolled over in contact with the wheels, and which, being preserved bright, does not corrode, being 1·75 inch wide, which leaves a net surface of corrosion of 2·44 square feet per yard for the rails in use, and of 2·88 square feet per yard for the rails out of use as above.

Taking the results of Table No. 4, therefore, we are enabled to deduce Table No. 5, which gives the corrosion in each case per square foot of surface of rail, and these results are then comparable with those of Table 15 of my Third Report on the Corrosion of Iron and Steel, &c., Transactions of British Association, and indeed comparable (by the aid of the standard bar as referred to in those reports) with all other results as to corrosion detailed therein, so that these experiments as to the corrosion of railway bars, may be hereafter extended or applied by others to rails rolled of any other make of iron whatever.

TABLE No. 5.

Nature of action on the rail.	First Experiment, Dublin and Kings- town Railway.	Second Experiment, Dublin and Kings- town Railway.	Third Experiment, Dalkey Railway.
	grs. avoird.	grs. avoird.	grs. avoird.
Corrosion out of use per square foot of rail per annum. . . . }	213·38	76·00	96·18
Corrosion in use, or exposed to traffic per square foot of rail per annum }	103·04	32·87	83·53
Differences	110·34	33·13	12·65

Thus again the differences show a constantly descending series.

If we extract from Table 15, Third Report on Iron, British Association, a few of the amounts of corrosion there given per square foot of surface, and reduce them to a period of one year, the foregoing corrosions will appear *in all instances remarkably less*.

The results of Table 15 are, however, not *strictly* comparable, as the iron there was exposed to the air and moisture of the City of Dublin, where the smoke and vapours, and excess of carbonic acid, close to the roofs and

chimneys, accelerate corrosion; still the difference is so remarkable, as to induce the suspicion, that there are some forces engaged which more or less retard the corrosion of iron when exposed in railway bars, in every condition, *i. e.* whether travelled over or not. Thus in one year, the losses by corrosion on one square foot of surface of the following sorts of iron exposed in the City of Dublin, were—

	grs. avoird.
2. Common Shropshire bar	1514.16
3. Best Staffordshire bar	1013.04
4. Best Dowlais Welsh bar	990.00
5. Low Moor boiler plate	932.40
8. Faggoted scrap bar	622.80

We now return to detail the examination made of the rails when just removed from the Kingstown Railway, as to their magnetic condition after the first exposure thereon, and also after their exposure upon the Dalkey line.

For this purpose the railway bars were brought into an open piece of level ground, remote from any masses of iron or other causes of magnetic disturbance; and at some considerable distance from where they lay, a triangle and purchase-blocks were so arranged, that any one bar could be suspended by the middle of its length in a horizontal position; the length of the bar being preserved either in the magnetic meridian or at right angles to it, or could be tilted up vertically, or in the line of the dip.

Each bar was first placed horizontally in the magnetic meridian by a line parallel therewith, previously marked out on the ground by two distant objects. A Kater's compass of delicate adjustment was now brought to rest, and then advanced slowly parallel with the bar towards the end facing the magnetic north, and the action of the bar on the needle noted. The compass was then brought back to the southern face or end of the bar, and the action also noted. The bar was then turned horizontally end for end, and similar experiments made; and lastly, the bar being turned round 90°, and thus being at right angles to the magnetic meridian, similar trials were made for each end. By this means the induced polarity by terrestrial magnetism was made evident, and separated from the idio-polarity, or the magnetism permanently proper to the bar.

Lastly, each bar was examined as to the position of the neutral point or points betwixt the poles, by carrying the compass slowly along its length while the bar was at right angles to the magnetic meridian, and observing when the needle was neither deflected to the east nor to the west, but continued to point to the magnetic north station mark, the point opposite to which was the neutral one. With a longer needle of greater delicacy, and suspended within a glass cylinder from silk fibres, examination was made as to the state of polarity of each bar, with reference to the depth of the rail, the top edge while in use being always uppermost and the bar horizontal.

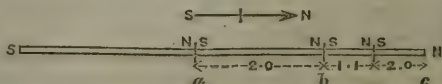
These experiments were made for every bar; it will not be necessary to describe the individual results in detail, but give them generally.

The set No. 1, not travelled over, and exposed to corrosion alone.—When examined after exposure on the Kingstown line, they showed strong polarity by terrestrial induction, but very slight idio-polarity. Some of the bars, however, when placed in the magnetic meridian, showed a feeble permanent polarity, reverse to that of

Fig. 7.

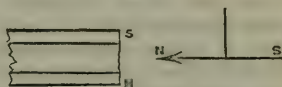
the earth, *i. e.* to induction, and more than one neutral point; one bar, for example, showed three neutral points at *a*, *b* and *c*, and

hence had eight poles with reference to length.



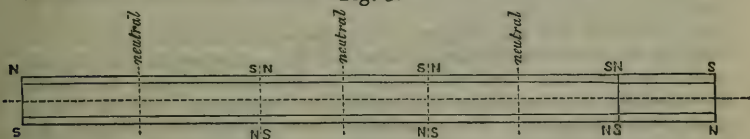
When this bar and some others were turned at right angles to the magnetic meridian, and examined with the suspended needle moved vertically up and down before their extremities, the existence of two poles at each end of the bar was made evident; thus the needle was quiescent at a certain point in the depth of the bar when held thus opposite the end of it, but was attracted or repelled in opposite directions when above or below this point.

Fig. 8.



This fact was subsequently ascertained to apply to all the railway bars; that is to say, a railway bar when polar is a magnet of such thickness, that it presents poles at its solid angles, not only with reference to length, but to depth, these poles being always of unequal intensity, the bar being in fact in the predicament of a cube or large parallelopiped of iron when exposed to magnetic induction. Hence the bar last adverted to with three neutral points had in fact sixteen poles distributed along its length; eight on the top and eight on the bottom flange or edge, and alternately of greater and of less intensity along the same edge, thus:—

Fig. 9.



These secondary poles, or those of depth, were not altogether due to terrestrial induction, as they preserved their signs, though with diminished intensity, when the bar was turned upside down, *i. e.* when the top edge became the lower, the direction and ends of the bar remaining unmoved, but they are probably due to induction within the bar itself.

Similar phenomena to the above present themselves in the same bars when examined after removal from the Dalkey line, as was to have been expected, there being no change in their condition in either case, *viz.* not having been travelled over on either line.

The set No. 2, not coated, and exposed both to oxidation and to abrasion of traffic, when examined in the same way, after experiment on the Kingstown line, all proved to be powerfully magnetic with polarity; the idio-polarity being almost in every case sufficiently intense to neutralize or reverse the polarity of terrestrial induction. In almost every instance there were two well-defined poles at the extremities of the bars, with one neutral point between.

In every rail the S. pole of the bar was found in the direction in which the traffic came in upon it, and the N. pole at that at which the traffic rolled off from it. Now the traffic passed over these bars in a direction from S.E. to N.W., and hence the direction of permanent polarity conferred upon the bar, coincides with the direction (in this instance at least) in which the traffic rolled over it.

The same bars when examined after removal from the Dalkey line (where it will be remembered the traffic is in both directions from W.N.W. to E.S.E. and *vice versa*, and the polarity of the bars acquired on the Kingstown line having been afterwards destroyed by heating them all to redness), were found powerfully idio-polar; some of the bars much more so than in the former case, so as to be able, when brought into the direction of dip, and thus

terrestrial induction made to aggrandize the idio-polarity, to lift and sustain a common sewing-needle; they had all two well-defined poles at the extremities and one neutral point, but the poles were seldom of quite the same intensity.

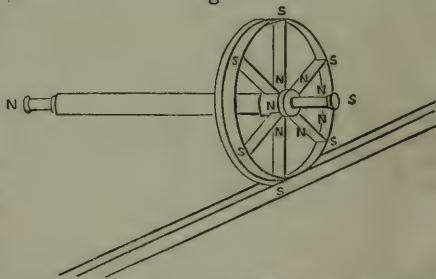
The set No. 3, viz. those coated with coal-tar and exposed to abrasion of traffic only, when submitted to a similar examination, present quite the same characters; each bar was powerfully idio-polar, with two well-defined poles of slightly unequal intensity and one neutral point. All the phenomena of terrestrial induction could of course be made evident with these as well as the other sets.

What is very remarkable, the intensity of magnetic polarity was quite as great in those bars and in the set No. 2 after removal from the Dalkey line, where they were travelled over in both directions, as after removal from the Kingstown, where travelled over only in one. In the latter case however the direction of the poles of each bar, as it lay in the line of railway, was that due to terrestrial induction, *i. e.* the south pole of the bar faced the north pole of the earth and *vice versa*; the opposite was the case with the bars on the Kingstown line of set No. 2, as already detailed.

These facts are sufficient wholly to overturn the views suggested by Mr. Nasmyth, in his letter already given, as to the peculiar effects of traffic in one or in both directions.

In connexion with this subject, I have also examined the magnetic condition of old wheels and axles that have run for a considerable time on railways. In every case these ran more or less in both directions; the results presented are very perplexing and variable, but in general I find the axle is more or less idio-polar. The wheel also is very often feebly idio-polar, the poles being at the nave and periphery; but when a pair of wheels and their own axle stand on the rails in the usual position, the axle being idio-polar, then by induction from the axle and by terrestrial induction conspiring, a tolerably distinct polarity of the whole wheel is produced, the nave presenting a pole opposite to that of its own end of the axle, and a pole opposite to this being found opposite the extremity of every spoke round the wheel-tire or periphery: thus when the wheel is rolled along the rails where the latter have been long in use and are themselves polar, the intensity varies with the position of the wheel over a given length of rail, and may be a maximum when the wheel rests over a joint between two rails, *viz.* over the polar extremities of the contiguous rails.

Fig. 10.



I endeavoured to ascertain whether the total intensity of the six bars constituting the set exposed to abrasion only, or of the six bars exposed to abrasion and corrosion, was the greater, but not having a suitably mounted magnetometer, I was unable to satisfy myself fully; so far as my trials went, however, there seemed but little difference appreciable in either case, and that equally so whether the two different sets were rolled over both ways, or only in one direction.

I ascertained also that the polarity of each bar, as it lies in the line of railway, is somewhat increased in intensity by induction from the bar lying parallel to it in the opposite side of the railway line. This effect however

is greater or less with the same breadth of gauge, dependent upon the position of the meeting-points of the ends of the rails, which are sometimes nearly opposite across the line, but often not so, *i. e.* the pole of one length of rail is opposite nearly to the neutral point of the rail at the other side of the railway.

It is manifest from what precedes, that the polar intensity of any given rail in a line of railway depends, not only upon the rolling traffic it is exposed to, but also upon the direction of the line of railway itself; that the bars in railways running north and south are in a higher state of magnetism, other things being the same, than in those running east and west, by the effects of terrestrial induction.

In long lengths of railway, running east and west especially, but in some degree in every direction, there are constant thermo-electric currents traversing the rails from end to end, due to changes in atmospheric temperature, between day and night, &c.: such currents have been already noticed by Mr. William Barlow as perpetually traversing the wires of the electric telegraph, and such currents may occasionally be due to other causes of disturbance of the equilibrium of terrestrial electricity than those due to temperature.

But the passage of locomotive engines over railways is a cause of electric currents traversing the bars of a much more important and formidable character. If a galvanometer be placed in connexion with a rail forming part of a line of way, and also with the earth at some distance, or with the rails at a distant point in advance, powerful deflections are produced by the approach of a locomotive engine, or even by the rolling along of a heavy train without an engine; in the latter case the effects are comparatively feeble, and appear to be due to the repeated compressions and rendings of crystallized surfaces, to which the surfaces rolling and rolled on are subjected; an action shown by Becquerel and others, to be an efficient producer of electric disturbance; but in the case of the locomotive engine, a torrent of electric force is let loose and finds its way into the earth along the rails, which, from the imperfect contact of their junctions, permit it to pass along the line with difficulty, and thus the equilibrium is gradually restored in great part through the chairs, fastenings and ballast, the resistance being greatest where the line is laid on longitudinal sleepers, and these are quite dry.

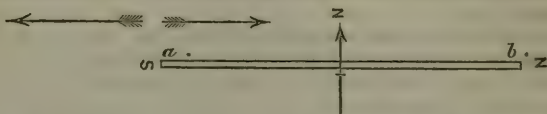
There can be little doubt, that if a portion of railway were insulated tolerably from beneath on wood, and a wood block inserted at given points, so as to cut off a segment of rails from the line, powerful sparks would flash from the rails and train as it passed over this portion of the railway. Indeed all this may be inferred from the well-known facts of the hydro-electric machine.

These electrical effects of locomotive engines in motion are somewhat uncertain, owing to the saline contents of the water usually employed frequently interfering; but after an engine has run a considerable distance without stopping, and the passages of efflux steam have become cleaned by its continued passage, and the engine does not prime, the evolution of electricity is always considerable.

As each railway bar may be viewed in the light of a conductor through which currents of electricity of variable intensity and quantity are perpetually flowing, it is obvious that magnetic currents are also in constant circulation round each bar and at right angles to its length: thus if upon the bar *a, b* an engine run over from south to north, the north end of a magnetic needle placed above the rail will be deflected to the left-hand, and *vice versa*.

Again, on the other hand, the existence of polarity of variable magnetic intensity in every rail involves the existence of electric currents circulating

Fig. 11.



round the bars, in accordance with the facts pointed out so well by Dr. Faraday. And in accordance with the recent experiments of Mr. Grove, the constantly recurrent induction of magnetism of great intensity in each rail involves a constant change of temperature in the rail, due to this cause alone, and probably an equally constant change in the molecular arrangement of its particles.

Such are the facts, so far as I have been enabled to observe them, of the complex relations to electricity, magnetism, and terrestrial temperature of railway bars; they fail to throw any direct light upon the immediate subject of our inquiry, but since the closest relationship has been proved to subsist between all these molecular forces, and especially since the later refined researches of Faraday and Plücker have shown that changes in the electrical or magnetic state of solids is attended with an instantaneous change in the relative groupings of their molecules, and knowing beforehand that chemical action in its most ordinary circumstances is powerfully influenced and modified by the state of grouping, or of aggregation of these molecules, it seems by no means improbable that the chemical action of air and moisture upon the iron of railway bars may be more or less modified by the electrical and magnetic forces that specially apply to them. To reduce this to certainty, demands experiments conducted, not after the manner or with the immediate object of those before us, but by refined research in the physico-chemical laboratory.

Interesting as such researches may be to science, and to which the facts here recorded may perhaps serve as finger-posts at the commencement of the way, they are not of very high value to the practical railway engineer, inasmuch as we have already found that the destruction of railway bars by corrosion is small in comparison with that by traffic. Nor are we obliged to rest in any vague speculation to find efficient causes sufficient to account for the real difference that we have established between the corrosion of the same railway bar in use and out of use.

The three principal causes to which I attribute this difference are,—

1st. The top surface of a railway bar in use is constantly preserved in a state of perfect cleanness, polish, and freedom from oxidation, while the remainder of the bar is rough, coated originally with black oxide (Hammerschlag) and soon after with red rust (peroxide and basic salts).

Not only is every metal electro-positive to its own oxides, but, as established in my Second Report on the Action of Air and Water on Iron (sec. 242), a mass of metal, partially polished and partly rough, is primarily corroded on the rough portion. Hence then a railway bar while in use is constantly preserved from rusting by the presence of its polished top-surface. Such polished surface has no existence upon the rail out of use.

2nd. The upper portion of the rail in use is rapidly condensed and hardened by the rolling of traffic over it; and I have also shown in the same reports, that all other circumstances being the same, the rate of corrosion of

any iron depends upon its density, and is less in proportion as this is rendered greater by mechanical means.

3rd. As every metal is positive to its own oxides, the adherent coat of rust upon iron, while it remains, powerfully promotes the corrosion of the metal beneath, and this in a greater degree in proportion as the rust adherent is of greater antiquity, inasmuch as it has been shown that the rust produced by air and moisture, which at first contains but little peroxide (Fe_2O_3), continues to change slowly, and becoming more and more peroxidized, becomes more and more electro-negative to its own base.

Now the rust formed upon a railway bar out of use continues always to adhere to it, and thus to promote and accelerate its corrosion, while the rust formed upon a railway bar in use is perpetually shaken off by the vibration of traffic, and thus this source of increased chemical action is removed. Of the extent to which this acts, we are informed by the results of the second experiment on the Kingstown Railway, where the weight of adherent rust, formed on removal of the bars out of use, was found to be more than four times as great as that upon an equal surface of the bars that had been in use.

We have found that this difference of corrosion in and out of use however is a constantly decreasing one; this arises from the fact that the condensation of the top-surface of the rails ceases after it has reached a certain point dependent on the maximum weight of the trains, and that after the lapse of a considerable period a uniform coat of rust is formed upon the rails in use, which is so firmly adherent that the vibration and wind of passing trains are incapable of sweeping it away; and it seems possible that after the lapse of an extremely prolonged time, the difference between the corrosion of the rail in use and out of use might become so small as to be perhaps insensible.

To recapitulate, then, we have found that railway bars forming part of a long line, whether in or out of use, corrode less for equal surfaces than a short piece of the same iron similarly exposed; that the rails in use do corrode less than those out of use; that this difference is one decreasing with the lapse of time; that the absolute amount of corrosion is a source of destruction of the rails greatly inferior to that due to traffic; that it is highly probable that the electrical and magnetic forces developed in the rails by terrestrial induction and by rolling traffic, react in some way upon the chemical forces concerned in their corrosion; and that therefore the direction of lines of railway in azimuth is not wholly indifferent as respects the question of durability of rails.

I am not aware of any information upon this subject having any character of accuracy which I can refer to extraneous to the present report. In the Franklin Journal, and also in Silliman's Journal, some few papers occur giving rather long statements as to the wear of rails on the Lowell and other American railways, as also some such in the Mining Journal (London), as to some English lines; as also some observations upon the abrasion of cast-iron rails, given by Thompson in his 'Colliery Inventions and Improvements.' They need not however be extracted here.

I might also extend this report to a comparison of the relations which subsist between the surface per yard lineal exposed to corrosion, and the strength and stiffness of the several principal forms of rail in use upon one line; but this can so obviously be done by the engineer for his own purpose, that it seems needless. It is however an element of choice in the form of rails that appears heretofore to have been wholly neglected.

I conclude therefore with two practical suggestions, deducible from the information we have obtained, having for object the increasing the durability of rails, both as against traffic and corrosion. First. Of whatever quality of

iron rails are rolled, I would suggest that they should be subjected prior to use to an uniform course of hammer hardening all over the top-surface and sides of top flange of the rail. The effect of this would be two-fold; the rail will be stiffened without any material reduction in ultimate strength; its surface will be hardened and polished, and hence best calculated to resist corrosion and abrasion; and lastly, the direction of the principal axes of the crystals of the iron, or of its "fibre," will for a small depth be changed and brought perpendicular to the surface of the rail, by which the tendency to lamination by rolling traffic will be greatly reduced. It will occur to any practical engineer that machinery may be constructed with perfect facility by which this hammer-hardening may be performed with rapidity and perfect smoothness and uniformity, the bar being slowly advanced, end on, under small hammers with suitably formed faces, driven rapidly by power. The total cost of the operation would amount to but a trifle on a ton of rails.

Secondly. I would suggest that all railway bars, before being laid down, should, after having been gauged and straightened, &c., be heated to about 400° Fahr. (but not higher, for fear of injuring the effect of the hammer-hardening), and then coated with boiled coal-tar; this I have proved in the preceding experiments to last for more than four years as a coating perfectly impervious to corrosive action while constantly exposed to traffic. The outlay for this would be very small; and if its value were no more than that after the lapse of eight or more years, when a set of rails had to be replaced in consequence of wear, the whole of the iron, which would have otherwise been dissipated in rust, would be returned to the furnace to be remade, the outlay would be well bestowed.

I would respectfully commend these suggestions to my professional brethren as worthy of trial, and have now fulfilled, so far as I have been able, the commands of the British Association, as to the corrosion and wear of rails.

Report on the Discussion of the Electrical Observations at Kew.

By WILLIAM RADCLIFF BIRT.

THE electrical observations made at the Observatory of the British Association at Kew from August 1, 1843, to August 8, 1848, are divisible into two portions, one occupying a period of seventeen months, viz. from August 1843 to December 1844 both inclusive, during which the readings were taken at sunrise, 9 A.M., 3 P.M. and sunset; the other, a period of three years and seven months, viz. from January 1844 to July 1848, also inclusive, during which the readings were taken at each even hour of Greenwich mean time as well as at sunrise and sunset. The last portion, which is by far the most complete, furnishes, from the observations of three complete years, the materials for deducing the diurnal and annual periods of the electrical tension. This has accordingly claimed our first attention and forms the first section of Part I., which is exclusively devoted to the examination of Positive Electricity.

The observations at sunrise and sunset, extending over the entire period of the five years, from the variability of the epoch of observation, require a separate discussion; they accordingly form the subject of the second section; and the third section is occupied with a discussion of the observations during the first seventeen months.

Scattered over the entire period of the five years we have several readings of negative electricity, and as they are evidently accompanied by meteorological phenomena of a peculiar and unmistakeable character which strongly indicate them to be the results of disturbances, rather than their forming any portion of a regular progression of the electric signs, they have also been separately discussed. Their discussion forms Part II. of this Report.

PART I.—POSITIVE ELECTRICITY.

SECTION I.—*Discussion of Positive Readings during the Years 1845, 1846 and 1847.*

During the years 1845, 1846 and 1847, 10,526 observations were recorded in the Journal, including the indications of the night-registering apparatus. Of these—

10,176	were positive;
324	„ negative, and
26	„ not employed in the discussion;
<hr/>	
10,526	

In the following table are recorded the twenty-six unemployed readings which were positive; they were in almost every case either preceded or succeeded by *negative* readings, from which it was concluded that they resulted more from a disturbance in the usual electrical condition of the atmosphere, than that they formed a part of its regular diurnal march: from these circumstances, connected with the high tensions mostly exhibited, it was apprehended the results would have been materially affected by employing them in the investigation.

In the following discussion, readings occasionally higher than some of those recorded below have been employed, but they have evidently formed either

a part of a regular diurnal movement, or have occurred at such hours as are generally distinguished by exhibiting an increase of tension. It was consequently considered that a rejection of them would to a certain extent interfere with the development of the diurnal and annual curves. The values in the table, as well as throughout the discussion, are recorded in terms of Volta's electrometer No. 1. The observations were taken with Henley's instrument, 1 degree of which has been approximately considered to be equal to 100 divisions of Volta No. 1*.

* On the 13th and 14th of July 1849, the reporter attended at the observatory for the purpose of comparing the electrometers, and especially determining the value of the readings of Henley's electrometer in terms of Volta's standard No. 1. The following are the results of the comparison. It appears from upwards of two hundred readings, the charge varying between 50 div. and 110 div. of Volta No. 1 as read from the scale of the No. 2 electrometer, that the mode of reading adopted by the observer at Kew, during the five years, was to bring the eye into such a position that the inner edge of the straw should coincide with the division read on the ivory arc of the instrument. By this mode of reading, 1° of Henley would very nearly equal in value 100 div. of Volta; this value has accordingly been retained, as most in accordance with the mode of reading adopted. It will be however evident that the true reading would be given, not by either *edge* of the straw, but by the *centre*: the diameter of the straw is equal to 2°; consequently when the inner edge coincides with 1°, the true reading must be 2°. From this it is clear that the values in the following discussion are relatively *too high*, but they will not interfere with the results further than by *expanding* the curves; the inflexions, points of maxima and minima, and the general form of the curves, will be the same, consequently the results derived from these curves will be unaffected. It would have been desirable to have applied a correction for this difference in the mode of reading, had not a greater difficulty presented itself in the dissimilarity of the construction of the two instruments, by which the values at different parts of the scale of Henley's instrument acquire different values in terms of Volta's instrument. The small extent of range common to both instruments renders it very difficult to express the higher readings of Henley at all accurately in terms of Volta. It is therefore considered best under the circumstances to retain the values as given in the tables, especially as the results are not materially interfered with; and endeavour to point out a mode by which the readings of Henley's instrument may be *accurately* expressed in terms of Volta, as well as to indicate a more precise method of observation.

The standard electrometer No. 1 of Volta is so constructed that a given electric force causes a pair of straws of a known weight to diverge. Their divergence is measured on a circular arc of the same radius as the length of the straws, which is so graduated as to indicate half the distance in arc between the extremities of the straws in half-Parisian lines, each of the divisions, which are at equal distances from each other, being equal to half a line. It is clear from this construction of instrument, that upon measuring the distance between the straws in a *right line*, the *sine of half the angle subtended by the extremities of the straws is proportional to the electric tension of the charge*.

The electrometer No. 2 is so constructed that each division is exactly equal to five of No. 1, and the circular arc is graduated to read at once the electric tension in terms of No. 1. The difference in the electrometers consists in the straws of No. 2 being heavier than those of No. 1, in such a proportion as to increase the value of the readings in the ratio above mentioned. As in No. 1 the sine of half the angle of divergence is proportional to the tension, so in No. 2 precisely the same value of the tension obtains, viz. the sine of half the angle of divergence, the linear value of the sine itself being proportional to its value in No. 1 for the same force: thus, a force that would diverge the straws in No. 1 to an angle of 30° would only open them in No. 2 to an angle of 6°, and in each instrument the sines of 15° and 3° respectively would represent the same force. There is however no necessity to employ such a determination of the force, the graduation of each instrument being amply sufficient for the purpose.

The Henley's electrometer is so constructed that the force is measured by a straw terminating in a pith-ball, which together constitute a pendulum that is inserted in a ball working by two fine steel pivots. This pendulum diverges by the electric force from the perpendicular, the angular amount of divergence being measured by a quadrant, graduated to degrees of the circle on an ivory scale. As it is thus used, the readings are not very readily comparable with those of the Volta's electrometers, in consequence of the Henley readings being in arc, while those of Volta are in linear measure. This difficulty may however be readily overcome by immediately measuring the *sine* of the angle of divergence, which in this instrument is a measure of the electric tension. Nothing further would be required than to place the electrometer in a convenient position for observation by a theodolite, which should be firmly fixed at a known distance from it. The centre of the azimuth circle should be in the precise verti-

cal plane of the centre of the pith-ball when unelectricified, and should be at such a distance that the arc measured by it may be of sufficient range to determine the length of the sine with tolerable accuracy. The distance between the centres of the azimuth circle and pith-ball should, if possible, be of such a value in half-Parisian lines as to facilitate the formation of a table for obtaining the value of the sine in half-Parisian lines by inspection, so that a simple observation of the bisection of the right and left limbs of the pith-ball, which of course would be in arc, and the deduced divergence in arc of its centre from its plane of rest when unelectricified, would, with the assistance of the table, give at once the electric tension in half-Parisian lines; and these readings might readily be converted into terms of Volta's electrometer No. 1, by properly adjusting the straw, pith-ball, &c. to a definite value, so that a divergence of half a Parisian line may be equivalent to a certain number of divisions of Volta's standard electrometer. In this way, it is clear, the tensions might be expressed in terms of Volta's standard up to 90° of Henley without the necessity of applying corrections, unless such corrections should be rendered necessary from the effects of gravity on the pendulum.

The whole matter may be rendered plain by the annexed diagram (fig. 1). Let A represent the centre of the pith-ball when unelectricified, and B the centre of the azimuth circle of the theodolite. The distance B A will form the base of a right-angled triangle, of which the divergence of the pendulum P—A' is the perpendicular. When the instrument is electrified, the pith-ball diverges in a plane at right angles to the plane passing through its centre when unelectricified, and that of the azimuth circle; or in other words, the plane of its motion passes vertically through the line A C, and is at right angles to the vertical plane passing through the line A B. If now the theodolite is so adjusted that the limbs of the pith-ball may be bisected, the azimuth circle will measure in arc the sine of the angle of divergence, and thus we have given the side and angles of a right-angled triangle from which the linear measure of the divergence may readily be deduced. The analogy is as follows:—Radius is to the tangent of the horizontal angle, as the distance between the centres of the pith-ball and azimuth circle is to the divergence.

Suppose the distance A B = 500 half-lines;
The azimuthal angle..... = 6° ;
Then Log A B = 2.698970
,, Log tan 6° = 9.021620

,, Log 52.55+ = 1.720590

Consequently the divergence is equal to 52.55 half-Parisian lines in a plane at right angles to the vertical plane passing through the above-mentioned centres.

N.B. The diagram is constructed in accordance with the above example.

It is not absolutely necessary to employ a theodolite. A telescope furnished with cross wires firmly fixed on a support having its centre of azimuthal motion at a known constant and invariable distance from the centre of the pith-ball when unelectrified, the angle being measured by an arm sufficiently extended to include the angle subtended by the pendulum when deflected from the perpendicular 90° , will be sufficient. A vertical motion should be given to the telescope by rackwork by which it can be raised to the level of the pith-ball when electrified, and it should be furnished with a level, &c. to ensure horizontality.

The above remarks have reference to the expression of the electric tension in the *linear* terms adopted by Volta, viz. *half-Paris lines*, and are principally applicable to the retention of Volta's notation so far as the measurement of the sine of the angle of divergence from the perpendicular is concerned; but Mr. Ronalds has suggested a much better mode of connecting the readings of the two instruments, viz. a conversion of the readings of Volta's electrometer (half Paris lines) into measures of *arc*, so that the readings of the three instruments, Volta No. 1, Volta No. 2, and Henley, and even of the discharger, may all be expressed in degrees of the circle, the sines of which are of course readily ascertainable.

TABLE I.
Unemployed positive readings.

Date.	Div. Volta No. 1.	Date.	Div. Volta No. 1.
1845. Feb. 23, 8 a.m.	2000	1846. June 25, 2 p.m.	4500
1845. May 20, 8 p.m.	3000	1846. Aug. 1, 4 p.m.	5500
1845. May 26, noon.	4500	1846. Aug. 1, 6 p.m.	5500
1845. June 4, 2 p.m.	3500	1846. Aug. 3, noon.	1500
1845. July 11, 2 p.m.	2000	1846. Aug. 5, 6 a.m.	3000
1845. Aug. 7, 2 p.m.	1000	1847. Mar. 10, 4 p.m.	2500
1845. Aug. 7, 4 p.m.	2000	1847. Apr. 29, 4 p.m.	2500
1846. Feb. 27, noon.	2000	1847. Apr. 29, 6 p.m.	1000
1846. Mar. 26, 6 p.m.	4500	1847. Apr. 30, 2 p.m.	2500
1846. Apr. 25, 2 p.m.	3000	1847. May 3, 2 p.m.	3000
1846. Apr. 26, 6 a.m.	2000	1847. July 17, 6 a.m.	2000
1846. May 6, 2 p.m.	4500	1847. July 17, 8 a.m.	3500
1846. May 20, 2 p.m.	5000	1847. Dec. 30, noon.	3000

DIURNAL PERIOD.

Diurnal period. Year.—In examining the results obtained from a discussion of the positive observations, it will be desirable to confine our attention first to the diurnal period of the electrical tension, or to those variations exhibited by the electrometers which have a day for the period in which they are completed, and which evidently depend on, or are connected with, the rotation of the earth on its axis.

The 10,176 observations upon which the mean diurnal period of the three years is based, are thus distributed among the twelve daily readings.

TABLE II.

Number of positive readings at each observation-hour in the three years 1845, 1846 and 1847.

Year.	Mid.	2 a.m.	4 a.m.	6 a.m.	8 a.m.	10 a.m.	Noon.	2 p.m.	4 p.m.	6 p.m.	8 p.m.	10 p.m.	Sums.
1845.	222	236	246	190	341	327	275	297	302	304	302	332	3374
1846.	234	257	269	190	353	338	288	278	287	281	286	338	3399
1847.	199	255	289	186	353	348	285	283	289	289	290	337	3403
Sums.	655	748	804	566	1047	1013	848	858	878	874	878	1007	10176

It will be remarked, that the greatest number of positive observations were recorded at 8 A.M., and the least number at 6 A.M. The numbers from noon to 8 P.M. do not vary materially in amount; but at 10 P.M. the number again increases. By consulting the following table of the distribution of negative observations, it will be seen that the greatest number occurred between 8 A.M. and 8 P.M. exclusive; this will to some little extent account for the difference; but the principal cause is, that on Sundays the observations were suspended between 10 A.M. and 10 P.M. exclusive. The small number of observations at 6 A.M. arises from the fact, that during the winter months, the *personal* observations were not commenced until 8 A.M., or more properly speaking until sunrise.

TABLE III.

Number of negative readings at each observation-hour in the three years 1845, 1846 and 1847.

Year.	Mid.	2 a.m.	4 a.m.	6 a.m.	8 a.m.	10 a.m.	Noon.	2 p.m.	4 p.m.	6 p.m.	8 p.m.	10 p.m.	Sums.
1845.	3	5	5	5	11	17	13	16	22	20	17	15	149
1846.	1	4	4	5	9	18	11	11	12	10	5	4	94
1847.	3	1	1	4	9	11	10	13	7	9	5	8	81
Sums.	7	10	10	14	29	46	34	40	41	39	27	27	324

The mode of discussion adopted has been, to arrange all the positive readings under the respective hours of observation, and then to divide their sums by the number of readings at each hour, so that the values recorded in the following tables are the arithmetical means of the readings at each observation-hour. The transcription from the Journal has been most carefully checked, and every precaution taken to ensure accuracy, both in ascertaining the number of observations and in calculating the means; in the latter case the arithmetical operations have been executed in duplicate. The results of these computations, as before mentioned, are expressed in terms of Volta's standard electrometer No. 1, all observations of tensions exceeding the range of this instrument having been reduced to its readings (see description of electrometers, 'Report,' 1844, p. 124, and the previous note on p. 114 of this Report).

On the 1st of January 1845, when the night-registering apparatus was first brought into use, a note occurs in the register which it is important to transcribe here. It is as follows:—

"The electric tensions at the hours 0, 2 and 4 are estimated by adding a quarter of a degree (of Volta) to the tensions exhibited by the three night-registering electrometers at sunrise, *for each hour which has elapsed between the time at which they were charged (by the clock) and the time of observing them (viz. sunrise).*

"The rate of loss by these electrometers begins to be inconstant after the tension has exceeded about 50° (of Volta): *vide* Experiments, 1844, p. ; if, therefore, the tension at sunrise of any such instrument shall exceed 50° , it is not noted in the Journal*."

TABLE IV.

Mean electrical tension at each observation-hour in the three years 1845, 1846 and 1847, with the mean diurnal period as deduced from the whole.

Year.	Mid.	2 a.m.	4 a.m.	6 a.m.	8 a.m.	10 a.m.	Noon.	2 p.m.	4 p.m.	6 p.m.	8 p.m.	10 p.m.	Mean.
	div.	div.	div.	div.	div.	div.	div.	div.	div.	div.	div.	div.	div.
1845.	19·8	17·8	18·3	28·6	64·7	84·4	67·9	59·9	59·2	71·1	98·9	117·2	63·1
1846.	24·3	21·2	21·4	35·4	61·1	76·7	69·6	65·5	63·5	85·0	96·3	87·2	61·3
1847.	23·7	21·1	21·5	38·7	78·7	102·5	88·4	89·4	85·0	99·1	112·0	107·9	76·3
Mean.	22·6	20·1	20·5	34·2	68·2	88·1	75·4	71·5	69·1	84·8	102·4	104·0	66·9

* For a full description of these night-registering electrometers, see 'Report,' 1844, p. 138, under the head of "Experiments on insulation by means of chloride of calcium."

TABLE V.

Excess or defect of the mean electrical tension at each observation-hour, as compared with the mean of the year, for the three years 1845, 1846 and 1847, and the mean diurnal period.

Year.	Mid.	2 a.m.	4 a.m.	6 a.m.	8 a.m.	10 a.m.	Noon.	2 p.m.	4 p.m.	6 p.m.	8 p.m.	10 p.m.	Mean.
	div.	div.	div.	div.	div.	div.	div.	div.	div.	div.	div.	div.	div.
1845.	43·3	45·3	44·8	34·5	1·6	21·3	4·8	3·2	3·9	8·0	35·8	54·1	63·1
1846.	37·0	40·1	39·9	25·9	0·2	15·4	8·3	4·2	2·2	23·7	35·0	25·9	61·3
1847.	52·6	55·2	54·8	37·6	2·4	26·2	12·1	13·1	8·7	22·8	35·7	31·6	76·3
Mean.	44·3	46·8	46·4	32·7	1·3	21·2	8·5	4·6	2·2	17·9	35·5	37·1	66·9

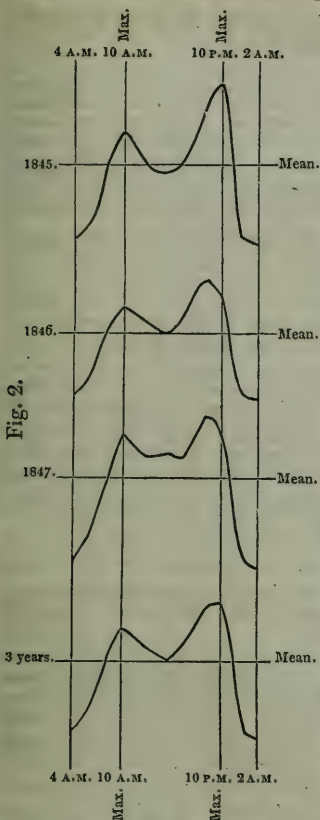
The above tables, which are based upon the numbers in Table II., clearly exhibit a double progression of the electrical tension during the twenty-four hours. The means of the first two years closely approximate, and in connexion with the general course of the numbers, give a proportional confidence, both with regard to the care manifested in making the observations and the faithfulness of the record. The third year exhibits upon the whole a higher tension, the means at midnight and 2 A.M. being the only values that are lower than those of the same hours in 1846. The mean of all the observations is 66·9 divisions of Volta's electrometer No. 1.

There are only three exceptions to the general fact, that from 8 A.M. to 10 P.M. the mean electrical tension is above the mean of the year. The mean diurnal period, as deduced from the three years, does not exhibit any depression *below* the mean of all the observations between the above-mentioned hours. The hours that exhibit a depression below the mean are midnight, 2, 4 and 6 A.M., and these are considerably in defect. The hour of minimum tension appears to be 2 A.M., a gradual rise taking place from that hour until 6 A.M. Between the hours of 6 and 8 A.M. a rapid rise occurs, the tension being nearly doubled; it then increases gradually until 10 A.M., when a maximum is passed, after which it gradually declines until 4 P.M., the epoch of the *diurnal* minimum as contradistinguished from the *nocturnal* minimum. The tension then rapidly increases until 8 P.M., and at 10 P.M. passes another maximum rather considerably above the maximum of 10 A.M. From 10 P.M. to midnight (two hours) the diminution of the tension is enormous, 81·4 divisions of Volta No. 1. The midnight value is but slightly above the value at 2 A.M., the epoch of the minimum.

The diurnal march of the tension is rendered more apparent to the eye by the annexed curves (fig. 2). The general similarity of the movements in the three years, and the close agreement between the curves of these years, and that of the mean diurnal curve as deduced from them, is, to a certain extent, satisfactory. The forenoon maximum is well marked in each case, as well as the evening maximum: in 1846 and 1847 this occurred at 8 P.M., and it may be probable that 9 P.M. may be the hour at which it is most frequently exhibited.

The lower readings at midnight, 2, 4 and 6 A.M., demand particular attention. From the note above extracted (see page 117), we find that tensions at these hours, above 50 div. of Volta No. 1, do not enter into the discussion. It is not only highly probable, but the absence of records at these hours, when Henley's electrometer has ranged rather high, indicates that the conductor

has possessed much higher charges than 50 div. at the hours of 0, 2 and 4.



The inference undoubtedly is, that the means at those hours are *too low*, and as a consequence, the mean of each year, as well as the mean of all the observations, is also *too low*. With regard to the hour of 6 A.M., the value appertains only to the summer, very few observations occurring at this hour in the winter. When we come to discuss the seasons, it will be seen that the higher tensions invariably occur in the winter; the value at 6 A.M., upon the whole year, is therefore also *too low*; consequently, were we in possession of either an uninterrupted series of *personal* observations during the day and night, or carefully executed photographic registers for the same period, we should doubtless have a curve which would exhibit neither so rapid a rise from 6 A.M. to 8 A.M., nor so great a fall from 10 P.M. to midnight, but would at these hours be more in accordance with its other portions. Of course it is important, in reference to this point, to bear in mind the circumstances under which the observations were made, the personal establishment not having enabled the observer to continue the observations during the night, and the uncertain diminution of the charges of the night-registering electrometers above 50 div. rendering it preferable *not* to record the indications of the instruments above 50 div., rather than insert numbers likely to vary from

the truth, and for which there are no certain means of correction. From a consideration of the tables and curves, it will be apparent, that the hour most suitable for observing the mean electrical tension during the entire year is 8 A.M., the difference from the mean at this hour being 1.3 div. in excess.

Diurnal period. Summer.—The 10,176 observations from which the diurnal period having reference to the entire year has been deduced, are thus divided:—

Summer.....	5,514
Winter	4,662
	<hr/> 10,176

The following table exhibits the distribution of the summer observations among the twelve daily readings: the months considered to constitute the summer half-year are, April, May, June, July, August and September:—

TABLE VI.

Number of positive readings at each observation-hour in the three summers of 1845, 1846 and 1847.

Year.	Mid.	2 a.m.	4 a.m.	6 a.m.	8 a.m.	10 a.m.	Noon.	2 p.m.	4 p.m.	6 p.m.	8 p.m.	10 p.m.	Sums.
1845.	135	135	147	176	174	167	135	146	149	152	158	171	1845
1846.	140	149	155	172	175	170	142	135	139	138	148	169	1832
1847.	125	148	163	177	178	173	139	137	141	140	147	169	1837
Sums.	400	432	465	525	527	510	416	418	429	430	453	509	5514

These numbers are more nearly equal in their amount than the yearly distribution.

TABLE VII.

Mean electrical tension at each observation-hour in the three summers of 1845, 1846 and 1847, with the mean diurnal period of summer.

Year.	Mid.	2 a.m.	4 a.m.	6 a.m.	8 a.m.	10 a.m.	Noon.	2 p.m.	4 p.m.	6 p.m.	8 p.m.	10 p.m.	Mean.
	div.	div.	div.	div.	div.	div.	div.	div.	div.	div.	div.	div.	div.
1845.	19.6	16.0	17.3	29.1	39.4	34.8	29.6	33.8	32.6	36.6	53.6	71.1	35.3
1846.	21.0	17.4	19.4	33.9	44.9	47.1	34.9	33.9	36.3	40.4	49.1	55.0	36.5
1847.	23.5	20.0	19.8	36.7	46.5	57.9	35.4	37.8	37.0	39.8	49.7	64.2	39.7
Mean.	21.3	17.8	18.9	33.2	43.6	46.7	33.4	35.1	35.2	38.9	50.8	63.4	37.2

TABLE VIII.

Excess or defect of the mean electrical tension at each observation-hour as compared with the mean of each summer in the years 1845, 1846 and 1847, and the mean of the three summers.

Year.	Mid.	2 a.m.	4 a.m.	6 a.m.	8 a.m.	10 a.m.	Noon.	2 p.m.	4 p.m.	6 p.m.	8 p.m.	10 p.m.	Mean.
	div.	div.	div.	div.	div.	div.	div.	div.	div.	div.	div.	div.	div.
1845.	15.7	19.3	18.0	6.2	4.1	0.5	5.7	1.5	2.7	1.3	18.3	35.8	35.3
	—	—	—	—	+	+	—	—	—	+	+	+	
1846.	15.5	19.1	17.1	2.6	8.4	10.6	1.6	2.6	0.2	3.9	12.6	18.5	36.5
	—	—	—	—	+	+	—	—	—	+	+	+	
1847.	16.2	19.7	19.9	3.0	6.8	18.2	4.3	1.9	2.7	0.1	10.0	24.5	39.7
	—	—	—	—	+	+	—	—	—	+	+	+	
Mean.	15.9	19.4	18.3	4.0	6.4	9.5	3.8	2.1	2.0	1.7	13.6	26.2	37.2

In contrasting the numbers in Tables VII. and VIII. with those in Tables IV. and V. having reference to the entire year, we are struck with the greater uniformity that prevails among those appertaining to the summer. The means approximate more closely to each other, the general course of the numbers is more regular, and the rise during the morning hours more gentle, although there is still a considerable diminution of tension between 10 P.M. and midnight.

In contemplating the numbers in Table VIII., indicating the excess or defect in comparison with the mean, we see at a glance that the double progression is well exhibited: at noon, 2 and 4 P.M., the numbers are in defect, or lower than the mean, as well as at midnight, 2, 4 and 6 A.M. It may be proper to mention here, that during the summer months the tension seldom

risers above 100 div. of Volta No. 1, except at particular hours; this will form a subject of discussion further on; in the meantime it enables us to gain some insight into the reason of the diurnal period during the summer months in each year being more in accordance with itself than that of the entire year. The defect of the early morning hours is not so great as the excess at 10 P.M.; consequently the mean line cuts the entire curve more equably, exhibiting the two maxima above, and the two minima below it. This doubtless arises from the very few tensions above 50 div. that occur during the summer nights, as well as from the observations at 6 A.M., which are generally low. We have therefore a period that differs but little, if any, from the natural progression of the electrical tension: 2 A.M. is the epoch of the principal minimum; the tension gradually rises from this hour until 10 A.M., the forenoon maximum; the succeeding minimum occurs at noon, the decline in the two hours being 13·3 div.; the rise is then very slow and gradual until 4 P.M., only 1·8 div.; at 6 P.M. the tension increases and mounts rapidly until 10 P.M., the principal maximum; the decline is then very considerable from 10 P.M. to midnight.

TABLE IX.

Comparison of the excess or defect from the mean of the diurnal periods deduced from all the observations, and from those made during the summer months.

Season.	Mid.	2 a.m.	4 a.m.	6 a.m.	8 a.m.	10 a.m.	Noon.	2 p.m.	4 p.m.	6 p.m.	8 p.m.	10 p.m.	Mean.
	div.	div.	div.	div.	div.	div.	div.	div.	div.	div.	div.	div.	div.
Year . .	44·3	46·8	46·4	32·7	1·3	21·2	8·5	4·6	2·2	17·9	35·5	37·1	66·9
Summer.	15·9	19·4	18·3	4·0	6·4	9·5	3·8	2·1	2·0	1·7	13·6	26·2	37·2

The above table places the diurnal period of the summer months in contrast with that of the entire year.

The annexed curves (fig. 3) exhibit the diurnal march of the tension during the summer months. The same similarity of movement is noticed as in the yearly curves; it is however worthy of remark, that the depression in or about the afternoon does not differ very essentially from that of the entire year, with the exception of the minimum occurring at noon. During the summer the evening maximum is 16·7 div. above the forenoon maximum, and during the entire year it is 15·9 div. The afternoon minimum is depressed below the evening maximum during the year 34·9 div., during the summer it is 30·0 div. This is in decided contrast with the lower branches of the curves, which exhibit a much greater difference. The difference of range in the two series of curves has not been exhibited, from the consideration that the nocturnal minimum of the entire year is probably too low.

Diurnal period. Winter.—The months constituting the winter half-year are, October, November, December, January, February and March. In the tables that follow, the means are not of consecutive months, but of January, February and March at the commencement, and October, November and December at the end of each year.

Fig. 3.

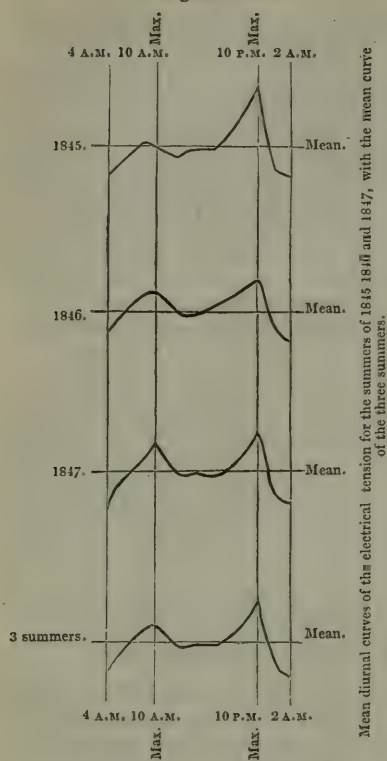


Fig. 4.

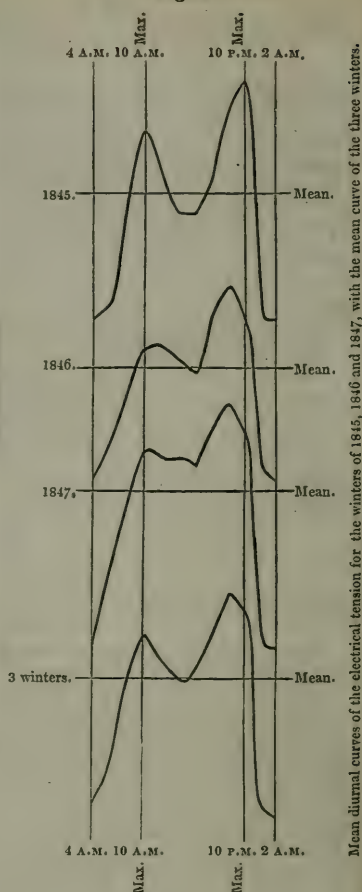


TABLE X.

Number of positive readings at each observation-hour in the three winters of 1845, 1846 and 1847.

Year.	Mid.	2 a.m.	4 a.m.	6 a.m.	8 a.m.	10 a.m.	Noon.	2 p.m.	4 p.m.	6 p.m.	8 p.m.	10 p.m.	Sums.
1845.	87	101	99	14	167	160	140	151	153	152	144	161	1529
1846.	94	108	114	18	178	168	146	143	148	143	138	169	1567
1847.	74	107	126	9	175	175	146	146	148	149	143	168	1566
Sums.	255	316	339	41	520	503	432	440	449	444	425	498	4662

TABLE XI.

Mean electrical tension at each observation-hour in the three winters of 1845, 1846 and 1847, with the mean diurnal period of winter.

Year.	Mid.	2 a.m.	4 a.m.	6 a.m.	8 a.m.	10 a.m.	Noon.	2 p.m.	4 p.m.	6 p.m.	8 p.m.	10 p.m.	Mean.
	div.	div.	div.	div.	div.	div.	div.	div.	div.	div.	div.	div.	div.
1845.	20.1	20.2	19.8	23.5	90.9	136.3	104.8	85.2	85.2	105.6	148.6	166.3	96.7
1846.	29.2	26.5	24.2	49.2	77.0	106.7	103.3	95.4	89.2	128.1	146.9	119.4	90.4
1847.	23.9	22.7	23.6	76.9	111.4	146.6	138.9	137.9	130.7	154.8	176.1	151.9	119.1
Mean.	24.5	23.2	22.7	46.5	93.1	130.0	115.8	106.0	101.5	129.4	157.3	145.5	102.1

TABLE XII.

Excess or defect of the mean electrical tension at each observation-hour as compared with the mean of each winter in the years 1845, 1846 and 1847, and the mean of the three winters.

Year.	Mid.	2 a.m.	4 a.m.	6 a.m.	8 a.m.	10 a.m.	Noon.	2 p.m.	4 p.m.	6 p.m.	8 p.m.	10 p.m.	Mean.
	div.	div.	div.	div.	div.	div.	div.	div.	div.	div.	div.	div.	div.
1845.	76.6	76.5	76.9	73.2	5.8	39.6	8.1	11.5	11.5	8.9	51.9	69.6	96.7
1846.	61.2	63.9	66.2	41.2	13.4	16.3	12.9	5.0	1.2	37.7	56.5	29.0	90.4
1847.	95.2	96.4	95.5	42.2	7.7	27.5	19.8	18.8	11.6	35.7	57.0	32.8	119.1
Mean.	77.6	78.9	79.4	55.6	9.0	27.9	13.7	3.9	0.6	27.3	55.2	43.4	102.1

Most of the remarks offered under the head of "*Diurnal period, Year,*" will equally apply to the present tables. There is, however, one feature that is very striking, viz. the greater range as well as amount of tension during the winter months, and that independent of the low readings during the early morning hours. The double progression is even more decided than in either of the former cases. In tracing the diurnal march we find the minimum at 4 A.M., a comparatively gentle rise takes place at 6 A.M., after which the tension rapidly mounts until 10 A.M., the forenoon maximum; it then gradually declines until 4 P.M., the afternoon minimum, and from this hour the rise is very rapid until 8 P.M., the epoch of the evening maximum. A fall of 11.8 div. takes place between 8 and 10 P.M., and then the enormous fall occurs between 10 P.M. and midnight, which we noticed in the yearly curves. The elevation of the evening above the forenoon maximum equals 27.3 div., and the depression of the intermediate minimum is as great as 55.8 div. The recess of the nocturnal maxima and minima from each other is interesting. The above phenomena are very clearly apparent in the annexed curves (fig. 4).

On contrasting these curves with those of the summer half-year (fig. 3), and comparing both with the curves having reference to the entire year on p. 119, the influence of the winter curves on those of the year is readily seen: the yearly curves present precisely the same general features as the winter curves. Taking this circumstance in connexion with the greater number of higher readings in winter than in summer, it may be inferred that the higher tensions materially influence the general results. The influence

of season in both instances, viz. the difference of tension and the form of curve, is very apparent from the series of summer and winter curves.

TABLE XIII.

Comparison of the excess or defect from the mean of the diurnal periods deduced from the observations in summer and winter.

Season.	Mid.	2 a.m.	4 a.m.	6 a.m.	8 a.m.	10 a.m.	Noon.	2 p.m.	4 p.m.	6 p.m.	8 p.m.	10 p.m.	Mean.
	div.	div.	div.	div.	div.	div.	div.	div.	div.	div.	div.	div.	div.
Summer.	15.9	19.4	18.3	4.0	6.4	9.5	3.8	2.1	2.0	1.7	13.6	26.2	37.2
Winter.	77.6	78.9	79.4	55.6	9.0	27.9	13.7	3.9	0.6	27.3	55.2	43.4	102.1

In the above table the summer and winter diurnal periods are placed in contrast.

TABLE XIV.

Synopsis of the principal points in the summer, winter, and yearly curves.

Season.	Nocturnal Minimum.	Forenoon Maximum.	Afternoon Minimum.	Evening Maximum.	Even. Max. above Forenoon.	Aftern. Min. below Even. Max.
Summer.	2 a.m.	10 a.m.	Noon.	10 p.m.	div. 16.7	div. 30.0
Winter ..	4 a.m.	10 a.m.	4 p.m.	8 p.m.	27.3	55.8
Year ...	2 a.m.	10 a.m.	4 p.m.	10 p.m.	15.9	34.9

The numbers in the last two columns clearly indicate a greater diurnal range of tension in winter than in summer; and this is very apparent from the curves, the upper portions of those of the winter being much bolder, and the depressions more distinctly marked, than the similar features of the summer curves. It is to be remarked, that although the diminution of tension between 10 P.M. and midnight is not so great in summer as in winter, the *precipitate* downward movement of the curve, which is so strikingly apparent in winter, does not in the summer disappear altogether, so as to give the curve that gentle depression to the nocturnal minimum which characterizes the rise from the afternoon minimum.

The three following tables exhibit the mean electrical tension at each observation-hour for each month in the years 1845, 1846 and 1847, with the monthly, seasonal, and yearly means. The characters of the monthly movements are exhibited to the eye in the sheets of curves illustrating this report.— See Plates VI. VII. and VIII.

TABLE XV.—Mean electrical tension at each observation-hour, as deduced from all the positive observations in the year 1845 arranged under the respective months, with the mean electrical tension of the summer, winter, and year.

Period.	January.	February.	March.	April.	May.	June.	July.	August.	September.	October.	November.	December.	Summer.	Winter.	Year.
Midnight..	14.5	22.2	22.0	22.3	19.4	16.7	16.0	21.4	22.3	19.1	28.1	19.6	20.1	19.8
2 a.m.	12.3	19.5	27.6	17.7	16.6	16.5	14.6	14.4	17.1	21.6	26.2	16.0	20.2	17.8
4 a.m.	18.2	18.9	21.6	18.0	18.2	20.3	15.5	15.8	16.4	16.2	27.2	17.3	19.8	18.3
6 a.m.	37.7	45.5	21.7	24.8	18.6	27.1	29.1	23.5	28.6
8 a.m.	113.8	197.4	51.5	56.5	50.4	32.4	22.1	25.1	49.6	46.9	71.5	69.6	39.4	30.9	64.7
10 a.m.	167.8	286.9	73.5	64.1	32.7	27.4	26.7	24.5	35.5	44.8	115.6	130.2	34.8	136.3	84.4
Noon	140.4	213.5	42.0	34.8	28.9	24.0	23.8	29.1	36.6	41.9	64.0	109.4	29.6	104.8	67.9
2 p.m.	125.9	139.3	46.5	36.2	41.4	24.2	25.0	31.9	43.7	46.8	84.0	70.2	33.8	85.2	59.9
4 p.m.	96.5	121.2	59.4	39.2	39.6	22.7	27.9	30.7	36.8	48.6	90.3	84.4	32.6	85.2	59.2
6 p.m.	139.4	191.8	55.4	43.0	32.2	27.9	38.2	47.2	47.2	51.4	92.3	96.8	36.6	105.6	71.1
8 p.m.	217.1	307.3	122.6	105.4	47.7	33.1	29.0	48.6	58.7	65.4	96.1	87.4	53.6	148.6	98.9
10 p.m.	136.8	395.6	153.0	143.7	78.8	39.5	49.6	61.7	57.3	94.4	112.1	108.2	71.1	166.3	117.2
Means.....	109.3	190.3	64.5	56.1	38.7	26.0	25.9	29.9	37.5	46.2	83.9	84.2	35.3	96.7	63.1

TABLE XVI.—Mean electrical tension at each observation-hour, as deduced from all the positive observations in the year 1846 arranged under the respective months, with the mean electrical tension of the summer, winter, and year.

Period.	January.	February.	March.	April.	May.	June.	July.	August.	September.	October.	November.	December.	Summer.	Winter.	Year.
Midnight..	32.5	31.3	33.0	27.4	23.1	21.1	22.1	17.9	18.0	26.1	24.3	32.8	21.0	29.2	24.3
2 a.m.	31.1	25.8	26.9	23.4	19.6	17.7	17.0	13.5	16.2	24.9	20.9	32.0	17.4	26.5	21.2
4 a.m.	22.4	31.4	23.8	21.6	21.1	23.1	22.5	14.0	15.2	21.3	19.7	31.5	19.4	24.2	21.4
6 a.m.	40.9	62.3	29.5	29.5	19.1	21.5	33.9	49.2	35.4
8 a.m.	100.7	92.8	80.2	84.2	53.4	41.1	36.4	27.9	30.7	56.9	49.6	82.3	44.9	77.0	61.1
10 a.m.	123.4	131.1	93.4	96.6	42.7	51.4	34.3	30.2	30.0	68.0	66.9	159.0	47.1	106.7	76.7
Noon	82.4	77.7	66.2	40.8	63.9	28.1	28.9	22.9	23.7	122.5	69.8	189.2	34.9	103.3	69.6
2 p.m.	101.6	69.1	66.1	38.3	42.3	23.0	30.3	44.8	25.0	59.1	59.2	205.0	33.9	95.4	65.5
4 p.m.	79.2	72.4	61.1	69.5	33.3	26.8	28.1	29.5	32.3	62.2	60.1	188.0	36.3	89.2	63.5
6 p.m.	143.4	133.3	83.7	73.1	36.9	37.8	34.1	31.9	33.7	77.4	58.8	270.3	40.4	128.1	85.0
8 p.m.	145.3	168.4	210.8	89.4	43.5	41.9	46.0	34.7	38.9	83.4	56.4	219.6	49.1	146.9	96.3
10 p.m.	133.8	158.9	98.1	98.3	61.5	48.0	44.7	34.5	43.1	107.5	54.3	167.2	55.0	119.4	87.2
Means.....	95.9	100.1	78.9	63.7	42.8	33.0	31.3	26.3	27.2	65.6	49.8	160.3	36.5	90.4	61.3

TABLE XVII.—Mean electrical tension at each observation-hour, as deduced from all the positive observations in the year 1847 arranged under the respective months, with the mean electrical tension of the summer, winter, and year.

Period.	January.	February.	March.	April.	May.	June.	July.	August.	September.	October.	November.	December.	Summer.	Winter.	Year.
Midnight..	29.3	32.5	35.5	31.1	19.3	25.2	30.4	20.6	20.2	17.6	19.7	24.5	23.5	23.9	23.7
2 a.m.	32.7	30.3	26.6	25.6	17.3	17.8	26.8	17.3	16.2	14.7	19.9	23.0	20.0	22.7	21.1
4 a.m.	34.1	26.8	22.3	22.8	15.6	23.4	25.0	17.4	15.7	19.2	18.8	27.6	19.8	23.6	21.5
6 a.m.	60.0	24.6	27.3	52.9	29.3	27.3	36.7	76.9	38.7
8 a.m.	185.2	160.0	135.1	76.7	35.5	29.6	56.9	40.0	39.2	62.3	70.9	52.4	46.5	111.4	78.7
10 a.m.	282.4	220.1	96.7	57.9	35.5	32.8	134.4	39.2	42.6	71.5	97.0	106.7	57.9	146.6	102.5
Noon	328.6	236.8	62.0	47.0	28.1	42.3	42.3	33.4	32.9	35.4	109.9	74.6	35.4	138.9	88.4
2 p.m.	331.0	266.9	48.2	55.5	34.9	28.5	47.3	29.6	32.0	33.4	91.1	83.3	37.8	137.9	89.4
4 p.m.	372.9	182.0	44.1	39.7	35.9	28.5	45.9	33.4	38.1	37.5	82.9	90.2	37.0	130.7	85.0
6 p.m.	333.8	229.3	77.2	46.4	38.5	27.0	47.1	37.2	42.0	43.9	140.8	116.8	39.8	154.8	99.1
8 p.m.	320.9	320.4	124.1	63.2	58.0	35.8	51.2	42.9	47.0	57.6	103.2	137.6	49.7	176.1	112.0
10 p.m.	263.6	259.9	133.6	72.5	42.6	41.9	126.2	42.3	55.9	50.2	88.7	127.0	64.2	151.9	107.9
Means.....	258.8	206.6	79.6	52.2	32.3	28.8	59.7	31.9	34.3	41.0	78.7	84.3	39.7	119.1	76.3

TABLE XVIII.—Mean electrical tension at each observation-hour, as deduced from all the positive observations in the years 1845, 1846 and 1847, arranged under the respective months, with the mean electrical tension of the summer, winter, and year.

Period.	January.	February.	March.	April.	May.	June.	July.	August.	September.	October.	November.	December.	Summer.	Winter.	Year.
Midnight..	23.0	27.1	30.2	26.6	20.6	21.2	22.1	20.0	20.1	20.6	22.5	28.1	21.3	24.5	22.6
2 a.m.	23.2	24.3	27.0	22.6	17.9	17.4	19.3	15.1	16.5	20.0	20.2	26.8	17.8	23.2	20.1
4 a.m.	23.5	25.7	22.6	21.1	18.2	22.3	21.1	15.7	15.7	18.9	19.3	24.6	18.9	22.7	20.5
6 a.m.	52.9	46.3	44.3	26.2	35.6	22.3	25.3	29.0	33.2	46.5	34.2
8 a.m.	134.7	149.4	91.5	72.5	46.4	34.4	38.6	31.1	39.8	55.5	63.9	68.5	43.6	93.1	68.2
10 a.m.	194.8	213.8	89.3	72.6	36.9	37.5	66.4	31.3	36.1	61.4	93.4	132.3	46.7	130.0	88.1
Noon	182.4	179.3	58.2	40.7	41.3	26.8	31.8	28.5	31.0	65.1	80.5	126.3	33.4	115.8	75.4
2 p.m.	183.0	156.8	53.4	42.8	39.6	25.2	34.3	35.4	33.6	46.1	71.4	122.3	35.1	106.0	71.5
4 p.m.	172.0	126.4	54.6	49.6	36.4	25.9	34.1	31.3	35.7	49.4	77.9	121.3	35.2	101.5	69.1
6 p.m.	203.7	186.4	72.3	53.2	35.8	30.9	37.5	35.7	41.0	57.2	97.3	161.3	38.9	129.4	84.8
8 p.m.	227.2	266.2	152.5	87.4	49.8	36.8	41.7	42.3	48.6	68.1	84.4	147.2	50.8	157.3	102.4
10 p.m.	178.2	271.6	126.1	105.1	60.7	43.2	73.5	46.3	52.4	83.0	84.2	134.1	63.4	145.5	104.0
Means.....	150.7	166.6	75.0	57.2	37.9	29.3	38.8	29.4	33.0	50.5	69.6	109.5	37.2	102.1	66.9

Table XVIII. exhibits the mean monthly electrical tension at each observation-hour deduced from the observations of *three* months, also the mean summer, winter, and yearly tensions deduced from the observations of *three* summers, winters, and years. The last line in the table to which the word "Means" is prefixed, exhibits the mean tension in each month as deduced from *all* the separate monthly observations; *i. e.* the mean tension of January, 150·7 div., is the result of all the January observations in the three years. The same thing holds good of the seasonal and yearly mean tensions.

The curves projected from these numbers will be found on Plate IX.

The tensions that enter into the preceding discussion range between 2 div. and 2000 div. in terms of Volta's standard electrometer No. 1. It has however been considered that tensions above 100 div. of this electrometer, or those measured by Henley's instrument, are not susceptible of that accuracy of determination which is requisite in the deduction of results, such as characterize those of modern science. In addition to this, it is apprehended that the electrical tension known more particularly as the tension of *serene weather*, seldom (if at all) rises above 100 div., although there may be movements indicated by Henley's instrument which partake of the character of those of serene weather.

In immediate reference to these points, and considerably elucidating them, remarks occur, either in the body of the Journal or in the notes and addenda accompanying it. In the description of the instruments at Kew published in the volume for 1844 (Reports, 1844, p. 124), the following occurs in reference to Henley's instrument:—

"This electrometer has seldom been observed until the Volta No. 2 had risen beyond 90° (in terms of the first, *i. e.* 18 lines \times 5); and since the uncertainty and difficulty of measuring the higher tensions increase in a rapid ratio with the increments of tension owing to unavoidable and sometimes almost imperceptible 'spirtings,' and particularly to the falling of rain from the dish or funnel N (fig. 2), proportionably less confidence must, of course, be placed in our notations of such tensions by means of this instrument."

In the account of the experiments having reference to the employment of photographic methods for self-registering the indications of the instruments, which is appended to the volume of observations 1845 and 1846, we have the following remark relative to the objection of the Astronomer Royal as to the non-registry of the *kind* of electricity:—

"I had not of course overlooked the objection as to not registering the kind of electricity, but as every former observer of the periodical electricity of serene weather, (*i. e.*) that alone which is susceptible of exact measurement, and that which is by far the most important and interesting, had arrived at the same conclusion as myself, (*viz.*) that it is positive, and that the exceptions to this law are extremely rare, and always accompanied by an easily distinguished feature in meteorology."

In the above extracts we have clearly a restriction of the electricity of *serene weather* to a comparatively low tension, and that the higher tensions, although more difficult to measure accurately, are not near so important as those which characterize serene weather. In immediate connection with this comparatively low tension we have the following remark, recorded on June 23, 1844:—

"The weather of this day, considered as serene, has been rather remarkable. The signs a little after sunrise were the highest *for such weather* that we have had. The thermometer *at nine* stood at 75·5, the max. also, and the barometer at 29·938. The atmosphere quite clear; the clouds were light,

rather fleecy, roundish and somewhat detached. Wind N.E. and E., its force about 500 grms. Daniell's hygrometer marked 20° of dryness."

At sunrise the electric tension was registered at 65 div. Volta No. 1. From this it appears that a tension of 65 div. at sunrise is considered as *high* for *serene weather*, and it might be inferred that tensions of a higher value indicated some other exciting cause than that which we contemplate as exciting the electricity of serene weather.

In the explanations and remarks concerning the Journal, &c. at Kew, published in the Report for 1844, p. 130, a serene day is defined as follows: "In the column N is pointed out (by the letter S) such days as generally occur when the positive charge rises after sunrise, falls early in the afternoon, and rises again in the evening, accompanied by what is commonly understood by the term 'fine weather'; but there are exceptions to this (rather vague) definition, which I believe require some habit, and an acquaintance with the observations of Monier and others, particularly Beccaria, to appreciate."

By glancing at the curves on pages 119 and 122, to which attention has been solicited, it will be seen that the movements, as deduced from the observations in individual years and seasons, as well as those from the entire number during the three years, are perfectly in accordance with the movements in serene weather, and it is only the restriction to which allusion has been made that suggests the probability of the higher tensions being due to a different exciting cause than that of the electricity of serene weather. In searching for such a cause among the records preserved in the Journal, we are struck with the fact, that in the majority of cases high tensions (*i. e.* those measured by Henley's electrometer) are accompanied by fog; and this suggests that it is not improbable that these high tensions may be more or less direct measures of the electricity, not of the atmosphere, but of the condensed aqueous vapour enveloping the collecting lanthorn. Of course the atmospheric electricity, as contradistinguished from that of the condensed aqueous vapour, will be mixed with it, and the conductor will be charged from two different sources, the atmospheric electricity exhibiting by far the smallest amount, and in cases of high charges forming probably but a very small proportion of the whole. There does not appear to be any direct means of separating these tensions; for if we take the *high* numbers, a small proportion, as we have already said, must appertain to "atmospheric electricity;" and if we take the *low* numbers as giving a more accurate measure of this element, on some occasions and especially at certain hours, the tensions exhibited may be those produced by the presence of aqueous vapour either in an invisible or condensed state, so that a degree of uncertainty as to the *true* forms of either of these diurnal curves must necessarily exist. Again, it is difficult to determine the point at which to separate the high from the low tensions; the uncertainty attendant on the readings of Henley's electrometer, combined with the electricity which alone is susceptible of exact measurement, tends greatly to place all readings of Henley's instrument in the category of *high* tensions. As a first attempt to separate the high from the low tensions, 1° of Henley equal to 100 div. of Volta No. 1 was regarded as the separating point; but it soon became apparent that readings lower than 100 div. had an equal claim to be regarded as high, indications being afforded that they were measures rather of the electricity of aqueous vapour than of the atmosphere. The observations of three or four months were discussed in this manner, but the curves of low tension presenting very anomalous characters, the mean readings increasing very considerably towards 8 P.M. led to their abandonment, and other separating points were tried from 50 div. and upwards. The result has been that the point 60 div. has been employed in the further discus-

sions of the observations, all readings above and including it being regarded as high, and more or less measuring the electrical tension of aqueous vapour either invisible or condensed; and all readings below it being regarded as more or less measuring the tension of "atmospheric electricity." Of course this method is entirely tentative; the separating point 60 div. has been arbitrarily fixed, and, as before observed, it is not to be expected that the curves furnished will be true representatives of natural phenomena, when we come to contemplate the two different sources from which the conductor is supposed to be charged; nevertheless it may not be without its use in assisting us to devise some mode by which the two tensions may be effectually separated, either by some subsidiary observations and computations by which the electrical tension of the aqueous vapour may be disengaged from the aggregate tension as exhibited by the electrometers, or by directly observing the electrical tension of the vapour itself.

TABLE XIX.

Mean diurnal period of *low* tension for the months of January, February, March and April 1845.

Period.	Jan.	Feb.	March.	April.
Midnight ..	14.5	22.2	22.0	22.3
2 a.m.	11.4	19.5	30.6	17.7
4 a.m.	18.2	18.9	23.6	18.0
6 a.m.	28.0
8 a.m.	34.1	41.0	49.8	40.3
10 a.m. ...	49.5	52.4	50.6	44.0
Noon	41.1	58.6	42.0	32.6
2 p.m.	41.1	57.2	49.7	37.3
4 p.m.	53.0	59.7	52.4	39.2
6 p.m.	46.7	61.8	50.2	42.2
8 p.m.	51.0	72.9	50.5	47.4
10 p.m. ...	44.3	49.5	48.4	43.0
Means	34.3	45.6	43.7	35.1

The above table and curves are intended to illustrate the separation of the readings into those of high and low tensions. The table contains the diurnal periods for the first four months of the year 1845, and the four upper curves are the projections of these periods on the same scale as the curves deduced from all the observations. The four lower curves exhibit the diurnal period for the same months as deduced from the readings below 60 div. The greater

uniformity of the lower curves, especially of January and February as compared with the upper, is very apparent. The curves appear naturally to divide themselves into two sets, the greater uniformity appertaining to January and February below 60 div., and to March and April, higher tensions than 60 div. entering as elements into the discussion of low ten-

sions. This seems at once to indicate the *variability* of any point that may be fixed on for the purpose of separating the two. Uniformity of curve clearly points out uniformity of action, and in endeavouring to obtain a knowledge of the action of the electricity of serene weather on the conductor and electrometers, it is to be presumed that it is to a great extent uniform and regular, and that consequently the curves will exhibit such uniformity and regularity among themselves. This then, in the absence of some direct means of measuring either the electricity of serene weather or of aqueous vapour, must be our principal guide in endeavouring to separate them; and although on some occasions greater uniformity may be obtained by either including or excluding particular tensions, yet upon the whole great uncertainty must prevail, if we attempt to *vary* the point of separation without more conclusive data than the mere uniformity of curve.

Diurnal period below 60 div., Year.—The 10,176 observations at all tensions are thus divided:—

Below 60 div.	7,529
Above 60 div.	2,647
	10,176

Those below 60 div. are thus distributed among the twelve daily readings.

TABLE XX.

Number of positive readings below 60 div. at each observation-hour in the three years 1845, 1846 and 1847.

Year.	Mid.	2 a.m.	4 a.m.	6 a.m.	8 a.m.	10 a.m.	Noon.	2 p.m.	4 p.m.	6 p.m.	8 p.m.	10 p.m.	Sums.
1845.	222	236	243	172	249	224	202	222	215	197	172	168	2522
1846.	234	257	267	170	235	214	212	195	201	171	148	181	2485
1847.	199	255	286	160	229	222	213	210	209	193	161	185	2522
Sums.	655	748	796	502	713	660	627	627	625	561	481	534	7529

From a consideration of the above quantities, we find that the greatest number of low tensions occurred at the hours 2, 4 and 8 A.M.; 6 A.M. appears to be excepted; but we must bear in mind that the number 502 refers principally to the summer half-year; with this exception, the smallest number of low tensions occurred at 6, 8 and 10 P.M. It is to be remarked that these periods coincide, more or less, with the principal epochs of minimum and maximum, the whole of the observations being taken into account.

TABLE XXI.

Mean electrical tension below 60 div. at each observation-hour in the three years 1845, 1846 and 1847, with the mean diurnal period as deduced from the whole.

Year.	Mid.	2 a.m.	4 a.m.	6 a.m.	8 a.m.	10 a.m.	Noon.	2 p.m.	4 p.m.	6 p.m.	8 p.m.	10 p.m.	Mean.
	div.	div.	div.	div.	div.	div.	div.	div.	div.	div.	div.	div.	div.
1845.	19·8	17·8	17·5	19·4	26·6	28·7	29·7	31·4	30·5	30·5	31·6	30·8	25·9
1846.	24·3	21·2	21·0	25·0	30·5	32·4	31·3	30·5	32·0	34·3	35·0	36·0	28·8
1847.	23·7	21·1	20·9	27·1	32·1	36·0	35·5	33·9	35·0	37·5	38·0	39·2	31·1
Mean.	22·6	20·1	19·9	23·7	29·6	32·3	32·2	32·0	32·5	34·1	34·8	35·5	28·6

TABLE XXII.

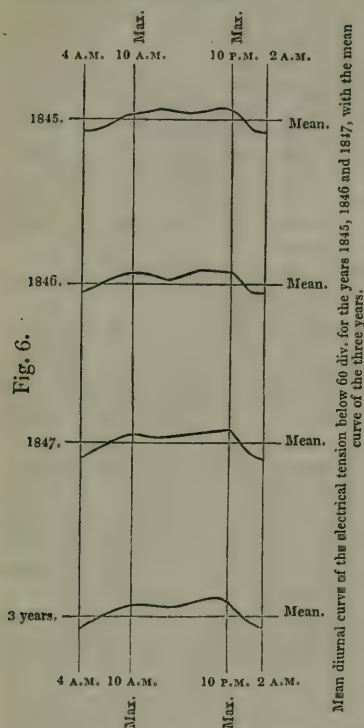
Excess or defect of the mean electrical tension below 60 div. at each observation-hour, as compared with the mean of the year for the three years 1845, 1846 and 1847, and the mean diurnal period.

Year.	Mid.	2 a.m.	4 a.m.	6 a.m.	8 a.m.	10 a.m.	Noon.	2 p.m.	4 p.m.	6 p.m.	8 p.m.	10 p.m.	Mean.
	div.	div.	div.	div.	div.	div.	div.	div.	div.	div.	div.	div.	div.
1845.	—	—	—	—	+	+	+	+	+	+	+	+	25·9
	6·1	8·1	8·4	6·5	0·7	2·8	3·8	5·5	4·6	4·6	5·7	4·9	
1846.	—	—	—	—	+	+	+	+	+	+	+	+	28·8
	4·5	7·6	7·8	3·8	1·7	3·6	2·5	1·7	3·2	5·5	6·2	7·2	
1847.	—	—	—	—	+	+	+	+	+	+	+	+	
	7·4	10·0	10·2	4·0	1·0	4·9	4·4	2·8	3·9	6·4	6·9	8·1	31·1
Mean.	—	—	—	—	+	+	+	+	+	+	+	+	
	6·0	8·5	8·7	4·9	1·0	3·7	3·6	3·4	3·9	5·5	6·2	6·9	28·6

In the above tables the double progression, so apparent in the curves deduced from all the positive observations, is but slightly developed. The forenoon maximum at 10 A.M. rises very slightly above the afternoon minimum at 2 P.M.—only 0·3 div. The evening and principal maximum occurs at 10 P.M., presenting the highest mean reading of the series. The year 1847 is marked by an increase in the low as well as in the aggregate tension, this increase appearing after the hour of 4 A.M. If the separation of the high from the low tensions at the point of 60 div. be that which is most accordant with truth, and the above tables exhibit more accurately the movements during serene weather than those which form the preceding part of this discussion, it would appear that upon contemplating the movements as deduced from the three years, there exists a great tendency to soften down or even to obliterate the forenoon maximum in such movements, so as to exhibit an approach to a single progression. The departure from an exhibition of the *true* march of the electricity of serene weather by the numbers before us, has been alluded to, inasmuch as the same cause, viz. the presence of aqueous vapour, must influence the results as deduced from the lower as well as those from the higher readings, and it becomes a curious matter of inquiry as to how far both the subdued maximum of the forenoon and the more decidedly developed maximum of the evening, in the progression of the lower tension, may be due to the presence of such vapour. It is a matter worthy of remark, and certainly is not without great signification, that the curves already discussed agree in presenting a precipitous downward movement between 10 P.M. and midnight. The tables now under consideration present in a very decided manner the same feature: although the extent of the diminution of tension is not so great as in the aggregate curves, yet as compared with the other two-hourly movements, it is sufficiently large to constitute a marked contrast to them, and this is by no means to be confined to the tensions we have hitherto examined; it will be found as we proceed to be an invariable accompaniment to nearly the whole of the curves.

The mean of the 7529 observations below 60 div. is 28·6 div., or 38·3 div. lower than the mean of the 10,176 positive observations. The minimum occurs at 4 A.M., from which hour the tension gradually rises until 10 A.M.; a very slight depression of 0·3 div. then takes place, the turning-point being at 2 P.M., from which hour the rise is very gradual until 10 P.M., the principal maximum, which is immediately succeeded by the precipitous diminu-

tion above mentioned. These phænomena are rendered more apparent by the annexed curves, fig. 6.



In the following table, the diurnal periods, as deduced from the aggregate observations and from those below 60 div., are placed in contrast.

TABLE XXIII.

Comparison of the excess or defect from the mean of the diurnal periods of the entire year, as deduced from the aggregate observations and from those below 60 div.

Value.	Mid.	2 a.m.	4 a.m.	6 a.m.	8 a.m.	10 a.m.	Noon.	2 p.m.	4 p.m.	6 p.m.	8 p.m.	10 p.m.	Mean.
	div.	div.	div.	div.	div.	div.	div.	div.	div.	div.	div.	div.	div.
Aggregate ...	—	—	—	—	—	—	—	—	—	—	—	—	—
	44·3	46·8	46·4	32·7	1·3	21·2	8·5	4·6	2·2	17·9	35·5	37·1	66·9
	—	—	—	—	—	—	—	—	—	—	—	—	—
Below 60 div.	6·0	8·5	8·7	4·9	1·0	3·7	3·6	3·4	3·9	5·5	6·2	6·9	28·6

Diurnal period below 60 div., Summer.—The 7529 observations below 60 div. are thus distributed in the two half-years:—

Summer	4846
Winter	2683
	7529

The following table exhibits the distribution of the 4846 summer observations among the twelve daily readings:—

TABLE XXIV.

Number of positive readings below 60 div. at each observation-hour in the three summers of 1845, 1846 and 1847.

Year.	Mid.	2 a.m.	4 a.m.	6 a.m.	8 a.m.	10 a.m.	Noon.	2 p.m.	4 p.m.	6 p.m.	8 p.m.	10 p.m.	Sums.
1845.	135	135	144	160	148	148	126	139	136	128	116	103	1618
1846.	140	149	153	158	138	139	137	129	129	121	102	119	1614
1847.	125	148	160	154	134	138	133	131	131	127	112	121	1614
Sums.	400	432	457	472	420	425	396	399	396	376	330	343	4846

This table exhibits a more equable distribution of observations over the twenty-four hours than that which has reference to the entire year; the greatest number occurs at 6 A.M., and the smallest at 8 P.M.

TABLE XXV.

Mean electrical tension below 60 div. at each observation-hour in the three summers of 1845, 1846 and 1847, with the mean diurnal period of summer.

Year.	Mid.	2 a.m.	4 a.m.	6 a.m.	8 a.m.	10 a.m.	Noon.	2 p.m.	4 p.m.	6 p.m.	8 p.m.	10 p.m.	Mean.
1845.	div. 19.6	div. 16.0	div. 15.8	div. 19.9	div. 24.5	div. 27.3	div. 26.6	div. 29.3	div. 28.4	div. 29.8	div. 31.7	div. 31.7	div. 31.7
1846.	21.0	17.4	18.7	24.4	29.0	30.1	27.5	27.0	28.2	31.2	31.2	33.3	26.2
1847.	23.5	20.0	18.9	26.9	32.3	35.6	34.0	32.2	33.8	35.4	37.4	39.6	30.3
Mean.	21.3	17.8	17.8	23.7	28.5	30.9	29.4	29.5	30.1	32.1	33.5	35.0	27.1

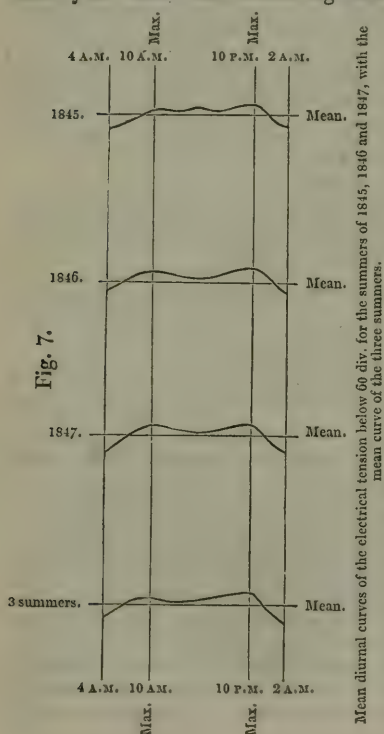
TABLE XXVI.

Excess or defect of the mean electrical tension below 60 div. at each observation-hour, as compared with the mean of each summer in the years 1845, 1846 and 1847, and the mean of the three summers.

Year.	Mid.	2 a.m.	4 a.m.	6 a.m.	8 a.m.	10 a.m.	Noon.	2 p.m.	4 p.m.	6 p.m.	8 p.m.	10 p.m.	Mean.
	div. —	div. —	div. —	div. —	div. —	div. —	div. —	div. —	div. —	div. —	div. —	div. —	div. —
1845.	5.1	8.7	8.9	4.8	0.2	2.6	1.9	4.6	3.7	5.1	7.0	7.0	24.7
1846.	5.2	8.8	7.5	1.8	2.8	3.9	1.3	0.8	2.0	5.0	5.0	7.1	26.2
1847.	6.8	10.3	11.4	3.4	2.0	5.3	3.7	1.9	3.5	5.1	7.1	9.3	30.3
Mean.	5.8	9.3	9.3	3.4	1.4	3.8	2.3	2.4	3.0	5.0	6.4	7.9	27.1

In these tables we find the forenoon maximum developed in a greater degree than in those having reference to the entire year—a result to be expected if the notion be correct that both low and high tensions are influenced by the presence of aqueous vapour in the atmosphere. The number of observations on which these tables are based forms a very considerable portion of the whole of the summer observations—rather above seven-eighths. The entire number is 5514, from which deduct those below 60 div., and we have left 668, or nearly an eighth part of the whole, so that the probability of the forenoon and evening maxima resulting from the presence of aqueous vapour is

rendered more apparent in the summer than during the entire year. It is important here to remark, that the results obtained by separating the summer observations from those of the entire year below 60 div. are of an opposite character to those obtained by dividing the aggregate observations into summer and winter series. In the case of the aggregate observations we found the summer curves representing the diurnal march, less in extent and less abrupt in their character than those of the entire year. On the contrary, we find the summer observations of low tension rather bolder in their character and of greater range than those of the entire year. In the former case, that of the aggregate observations, the summer readings were as a mass much lower than those of the winter; there were also a much greater number that would have especial reference to *serene weather* than of those in the winter, and these circumstances would *reduce* the summer curves to the form in which we find them. When however we contemplate the tensions below 60 div., there is nothing cut off in the summer from those furnishing the results of the year, the whole of the observations up to and including 59 div. finding entry at all seasons; but we have a much greater number of low tensions during the summer than in the winter, so that a greater portion of the entire phænomena is as it were compressed into the lower readings, and manifests itself by expanding the summer curves as compared with those of the entire year rather than contracting them.



In tracing the diurnal march of the tension below 60 div., we find the minimum occurring at 2 and 4 A.M.; after 4 A.M. the tension gradually rises until 10 A.M., the epoch of the forenoon maximum; a fall of 1.5 div. occurs between 10 A.M. and noon, after which a very gradual and regular rise takes place until 10 P.M., the epoch of the evening maximum, which is succeeded by the precipitous diminution of tension already alluded to. In the diurnal minimum occurring at noon, and its being followed by a gentle rise to the evening maximum, we have repeated to a certain extent the same feature which we noticed as characterizing the summer curve of the aggregate observations. There is however one important point of difference which strikingly exhibits the influence of the higher tensions on the curves: the hours of maxima and minima are nearly if not the same in both cases, and the gentle rise from noon to 4 P.M. in each instance possesses many features in common, the principal difference being a greater movement in the aggregate than in the low tensions. The point of differ-

ence to which we particularly solicit attention is the augmentation in the summer curves of low tension of the forenoon and evening maxima, and their contraction in the aggregate summer curves. This is very apparent on consulting the curves. In discussing the high tension during the summer months, this subject will be again referred to; in the mean time we may notice here, that upon the consideration of the high readings measuring the electrical tension of aqueous vapour, it appears probable that these maxima depend on the presence of aqueous vapour for their development.

TABLE XXVII.

Range of the diurnal curves of electric tension below 60 div. in the summers of 1845, 1846 and 1847, and also in each year.

Year.	Summer curve.	Yearly curve.
	div.	div.
1845.	15·9	14·1
1846.	15·9	15·0
1847.	20·7	18·3
Mean.	17·2	15·6

TABLE XXVIII.

Comparison of the excess or defect from the mean of the diurnal periods of summer, as deduced from the aggregate observations and from those below 60 div.

Value.	Mid.	2 a.m.	4 a.m.	6 a.m.	8 a.m.	10 a.m.	Noon.	2 p.m.	4 p.m.	6 p.m.	8 p.m.	10 p.m.	Means.
	div.	div.	div.	div.	div.	div.	div.	div.	div.	div.	div.	div.	div.
Aggregate ...	15·9	19·4	18·3	4·0	6·4	9·5	3·8	2·1	2·0	1·7	13·6	26·2	37·2
Below 60 div.	5·8	9·3	9·3	3·4	1·4	3·8	2·3	2·4	3·0	5·0	6·4	7·9	27·1

The above table places the aggregate and low tension summer diurnal periods in contrast.

Diurnal period below 60 div., Winter.—The following table exhibits the distribution of observations below 60 div. during the winter among the twelve daily readings.

TABLE XXIX.

Number of positive readings below 60 div. at each observation-hour in the three winters of 1845, 1846 and 1847.

Year.	Mid.	2 a.m.	4 a.m.	6 a.m.	8 a.m.	10 a.m.	Noon.	2 p.m.	4 p.m.	6 p.m.	8 p.m.	10 p.m.	Sums.
1845.	87	101	99	12	101	76	76	83	79	69	56	65	904
1846.	94	108	114	12	97	75	75	66	72	50	46	62	871
1847.	74	107	126	6	95	84	80	79	78	66	49	64	908
Sums.	255	316	339	30	293	235	231	228	229	185	151	191	2683

In this table, the greatest number of readings occur at 4 A.M., the epoch of the principal minimum, and the least number at 8 P.M., two hours after the evening maximum. It will be remarked, that the morning hours, viz. 2 and 4 A.M., exhibit the greatest number, and the evening hours; 6, 8 and 10 P.M., the least. The thirty readings at 6 A.M. are excepted, for the reason stated on page 130.

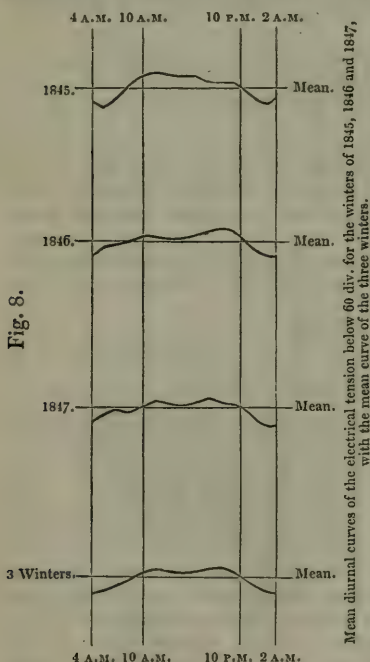
TABLE XXX.

Mean electrical tension below 60 div. at each observation-hour in the three winters of 1845, 1846 and 1847, with the mean diurnal period of winter.

Year.	Mid.	2 a.m.	4 a.m.	6 a.m.	8 a.m.	10 a.m.	Noon.	2 p.m.	4 p.m.	6 p.m.	8 p.m.	10 p.m.	Mean.
	div.	div.	div.	div.	div.	div.	div.	div.	div.	div.	div.	div.	div.
1845.	20.1	20.2	19.8	13.2	29.6	31.4	34.8	34.8	34.2	31.7	31.5	29.2	28.1
1846.	29.2	26.5	24.2	32.1	32.5	36.6	38.3	37.5	38.8	42.0	43.4	41.3	33.8
1847.	23.9	22.7	23.6	32.9	31.7	36.8	38.0	36.8	37.1	41.5	39.4	38.4	32.4
Mean.	24.5	23.2	22.7	24.7	31.2	35.0	37.1	36.3	36.6	38.0	37.7	36.2	31.4

TABLE XXXI.—Excess or defect of the mean electrical tension below 60 div. at each observation-hour, as compared with the mean of each winter in the years 1845, 1846 and 1847, and the mean of the three winters.

Year.	Mid.	2 a.m.	4 a.m.	6 a.m.	8 a.m.	10 a.m.	Noon.	2 p.m.	4 p.m.	6 p.m.	8 p.m.	10 p.m.	Mean.
	div.	div.	div.	div.	div.	div.	div.	div.	div.	div.	div.	div.	div.
1845.	—	—	—	—	+	+	+	+	+	+	+	+	28.1
1846.	8.0	7.9	8.3	14.9	1.5	3.3	6.7	6.7	6.1	3.6	3.4	1.1	33.8
1847.	—	—	—	—	—	—	—	—	—	—	—	—	32.4
1847.	8.5	9.7	8.8	0.5	0.7	4.4	5.6	4.4	4.7	9.1	7.0	6.0	32.4
Mean.	—	—	—	—	—	+	+	+	+	+	+	+	31.4
	6.9	8.2	8.7	6.7	0.2	3.6	5.7	4.9	5.2	6.6	6.3	4.8	31.4



In order to facilitate the comparison of the diurnal march of the low tensions during the individual winters, which present some striking features of interest, we shall at once introduce the curves to the notice of the reader. On contemplating them, it will be at once apparent that they present several interesting points of contrast. There appears to be a greater approach to a single progression, especially in the winter of 1845. In this curve the maximum occurs at noon and 2 P.M.; the precipitous diminution between 10 P.M. and midnight disappears, the curve taking a gently rounded course from 2 P.M. to midnight; there appears to be a slight check to this gradual diminution of tension at 8 P.M. The principal minimum occurs at 6 A.M., the rise from this hour to noon being of a bold, rounded character; it is probable that the true minimum occurs at 4 A.M., twelve observations only contributing to the determination of the value at 6 A.M. On contrasting this curve with those of the summer and entire year aggregate tension, we find the movements during the day reversed, the greatest development occurring about the middle of the day. A much

greater number of observations of high tensions contribute to the production of the aggregate curve in winter than in summer, and as a consequence, the observations on which the winter curve of low tension is based are less numerous than those on which the summer curve rests. In the curve now before us, the double progression may be considered if not entirely, as almost disappearing; the removal of the higher tensions appears to be accompanied by a removal of the forenoon and evening maxima, which is replaced by a maximum near the middle of the day. This is extremely striking when we compare our curve with that of the winter, as deduced from all the positive readings (page 122); in this curve the forenoon and evening maxima are strongly developed, and the depression at 2 and 4 P.M. very distinctly marked. It would appear, on the supposition of the high readings being measures of the electrical tension of aqueous vapour, that in this particular winter (1845), very few measures of such tensions occurred below 60 div., so that in the great majority of instances, the readings below 60 div. were, more or less, measures of atmospheric electricity. The curve itself suggests the inquiry—Is the diurnal march of atmospheric electricity—viz. that which is uncombined with the electrical tension characterizing, or developed by, the presence of aqueous vapour—a single progression? In other words, does the electrical tension of dry air present a curve having simply an ascending and descending branch, the progression being in harmony with the temperature? We shall have occasion to refer again to this subject in a future part of this Report.

On turning our attention to the winter of 1846, we find a curve more or less in harmony with those of the summer and entire year, and strikingly in contrast with that of the winter of 1845. It is however to be remarked, that the depression at 2 P.M. is but slight, and very much less than the depression during the summer of this year; the slight check which is apparent in the forenoon rise, at 8 A.M., tends to give the curve an appearance of possessing three maxima; there is indeed a great tendency to assume somewhat of the form of 1845, which appears to be counteracted by the greater development of the evening maximum.

The winter curve of 1847 may be characterized as exhibiting considerable trepidation, and consisting of alternate but very subordinate maxima and minima, the principal of which occurs at 6 P.M. There is an evident tendency to a single progression, having its maximum about the early afternoon hours. This curve is in contrast with that of the winter of 1845, inasmuch as the most rounded portion of the curve is developed in the evening.

On directing our attention to the mean of the three winters, we find two maxima, noon and evening, well-developed, but of a subdued character. The evening maximum is the principal; it however rises only 0·9 div. above that at noon; the intermediate minimum occurs at 2 P.M., and is depressed 1·7 div. below the principal maximum.

TABLE XXXII.

Synopsis of the principal points in the summer, winter, and yearly curves below 60 div.

Season.	Forenoon Maximum.	Minimum.	Evening Maximum.	Nocturnal Minimum.	Even. Max. above Forenoon.	Aftern. Min. below Even. Max.
Summer.	10 a.m.	Noon.	10 p.m.	2 & 4 a.m.	div. 4·1	div. 5·6
Winter...	Noon.	2 p.m.	6 p.m.	4 a.m.	0·9	1·7
Year	10 a.m.	2 p.m.	10 p.m.	4 a.m.	3·2	3·5

This subdued character of the two maxima, as well as the comparatively slight depression of the included minimum, is well seen in the above table; and when combined with the characters of the individual curves of each winter which have been noticed above, together with the approach of the epochs of maxima in the mean curve to each other, viz. from 10 A.M. to noon, and from 10 P.M. to 6 P.M., a strong probability is suggested, that were we able effectually to separate the high from the low tensions, *not at an arbitrary point*, but in such a manner that the high tensions of summer (in all probability lower than those of winter) should find entry in their respective department, the result would be, that the low tensions would exhibit a single progression in harmony with the temperature.

In the following table the diurnal periods for the winter, as deduced from the aggregate and low tensions, are placed in contrast.

TABLE XXXIII.

Comparison of the excess or defect from the mean of the diurnal periods of winter, as deduced from the aggregate observations and from those below 60 div.

Valuc.	Mid	2 a.m.	4 a.m.	6 a.m.	8 a.m.	10 a.m.	Noon.	2 p.m.	4 p.m.	6 p.m.	8 p.m.	10 p.m.	Mean.
	div.	div.	div.	div.	div.	div.	div.	div.	div.	div.	div.	div.	div.
Aggregate ..	77·6	78·9	79·4	55·6	9·0	27·9	13·7	3·9	0·6	27·3	55·2	43·4	102·1
Below 60 div.	6·9	8·2	8·7	6·7	0·2	3·6	5·7	4·9	5·2	6·6	6·3	4·8	31·4

Tables XXXIV., XXXV., XXXVI. exhibit the mean electrical tension at each observation-hour for each month in the three years 1845, 1846 and 1847, with the monthly seasonal and yearly means. The characters of the monthly movements are exhibited to the eye in the sheets of curves illustrating this report. See Plates VI. VII. and VIII.

Table XXXVII. exhibits the mean monthly electrical tension at each observation-hour deduced from the observations of *three* months, also the mean summer, winter, and yearly tension deduced from the observations of *three* summers, winters, and years. The last line in the table, to which the word "Means" is prefixed, exhibits the mean tension in each month as deduced from all the separate monthly observations, *i. e.* the mean tension of January, 31·5 div. is the result of all the January observations in the three years. The same thing holds good of the seasonal and yearly mean tensions.

The curves projected from these numbers will be found on Plate IX.

Previous to proceeding with the discussion of the high tensions, it will be advantageous to pause, for the purpose of recapitulating the principal points that have hitherto come under our notice, and of particularly directing our attention to those that stand out prominently from among the others.

1. We have seen that the discussion of the entire series of the positive observations for the three years furnishes us with series of curves, exhibiting in a most decided manner *a double progression*. The points of maxima and minima are well-marked, and in most cases they present a tolerable fixity of epoch.

2. The presence of *fog* mostly occurring on those occasions when *high* electrical tensions have been observed, combined with the opinion that the electricity of serene weather is mostly characterized by *low* tensions, has

TABLE XXXIV.—Mean electrical tension at each observation-hour, as deduced from all the positive observations *below* 60 div. in the year 1845, arranged under the respective months, with the mean electrical tension of the summer, winter, and year.

Period.	January.	February.	March.	April.	May.	June.	July.	August.	September.	October.	November.	December.	Summer.	Winter.	Year.
Midnight...	14.5	22.2	22.0	22.3	19.4	16.7	16.0	21.4	22.3	19.1	28.1	19.6	20.1	19.8
2 a.m.	12.3	19.5	27.6	17.7	16.6	16.5	14.6	14.4	17.1	21.6	26.2	16.0	20.2	17.8
4 a.m.	18.2	18.9	21.6	18.0	15.3	17.1	13.2	15.8	16.4	16.2	27.2	15.8	19.8	17.5
6 a.m.	22.0	21.2	15.7	19.0	18.6	23.2	19.9	13.2	19.4
8 a.m.	34.1	38.3	33.1	30.8	28.9	19.6	22.1	19.7	27.8	23.2	31.3	25.5	24.5	29.6	26.6
10 a.m.	37.7	34.9	38.1	37.4	29.1	22.1	25.5	22.5	31.5	28.4	34.5	20.9	27.3	31.4	28.7
Noon	38.2	40.8	31.3	29.9	27.1	19.7	23.8	25.6	32.8	33.0	40.0	27.8	26.6	34.8	29.7
2 p.m.	30.6	43.2	31.1	32.8	27.7	24.2	25.0	30.5	35.5	38.5	32.7	33.1	29.3	34.8	31.4
4 p.m.	34.4	40.7	32.7	33.7	26.1	22.7	27.9	30.7	31.0	33.9	36.9	28.2	28.4	34.2	30.5
6 p.m.	31.2	32.9	28.8	31.9	26.9	25.0	27.5	34.0	35.7	28.4	32.7	38.0	29.8	31.7	30.5
8 p.m.	32.9	41.9	32.8	41.4	38.3	28.9	25.1	29.1	33.4	23.4	31.4	35.0	31.7	31.5	31.6
10 p.m.	26.5	36.6	24.7	25.1	34.1	29.9	30.6	34.8	34.0	26.6	34.3	28.1	31.7	29.2	30.8
Means.....	25.8	30.7	28.7	29.0	25.2	21.5	22.5	24.0	27.5	25.6	32.4	28.6	24.7	28.1	25.9

TABLE XXXV.—Mean electrical tension at each observation-hour, as deduced from all the positive observations *below* 60 div. in the year 1846, arranged under the respective months, with the mean electrical tension of the summer, winter, and year.

Period.	January.	February.	March.	April.	May.	June.	July.	August.	September.	October.	November.	December.	Summer.	Winter.	Year.
Midnight...	32.5	31.3	33.0	27.4	23.1	21.1	22.1	17.9	18.0	26.1	24.3	32.8	21.0	29.2	24.3
2 a.m.	31.1	25.8	26.9	23.4	19.6	17.7	17.0	13.5	16.2	24.9	20.9	32.0	17.4	26.5	21.2
4 a.m.	22.4	31.4	23.8	21.6	19.0	23.1	20.8	14.0	15.2	21.3	19.7	31.5	18.7	24.2	21.0
6 a.m.	26.0	27.6	27.4	27.8	19.1	19.6	21.4	32.1	25.0
8 a.m.	29.8	26.0	34.9	35.2	34.4	29.1	31.7	23.6	24.3	33.7	30.7	44.4	29.0	32.5	30.5
10 a.m.	31.4	34.2	41.1	36.6	32.7	33.1	30.9	26.2	24.7	39.6	31.7	44.4	30.1	36.6	32.4
Noon	43.3	36.7	40.5	37.2	25.9	28.1	28.9	22.9	23.7	37.5	34.2	36.9	27.5	38.3	31.3
2 p.m.	41.6	41.5	42.8	34.6	26.0	23.0	30.3	25.2	25.0	31.4	36.5	34.2	27.0	37.5	30.5
4 p.m.	40.5	37.1	41.9	28.7	31.4	24.1	28.1	27.7	29.6	39.0	34.2	44.2	28.2	38.8	32.0
6 p.m.	44.7	43.1	43.1	40.6	33.8	29.2	34.1	27.5	24.6	37.1	42.8	38.3	31.2	42.0	34.3
8 p.m.	43.1	47.5	38.7	37.9	30.2	31.1	37.7	24.9	28.8	45.7	39.4	52.5	31.2	43.4	35.0
10 p.m.	37.5	36.9	43.3	31.0	31.6	37.7	38.0	29.9	29.6	41.9	41.3	46.7	33.3	41.3	36.0
Means.....	34.7	34.6	35.1	31.3	27.5	26.8	28.5	22.3	22.8	33.3	30.7	37.3	26.2	33.8	28.8

TABLE XXXVI.—Mean electrical tension at each observation-hour, as deduced from all the positive observations *below* 60 div. in the year 1847, arranged under the respective months, with the mean electrical tension of the summer, winter, and year.

Period.	January.	February.	March.	April.	May.	June.	July.	August.	September.	October.	November.	December.	Summer.	Winter.	Year.
Midnight...	29.3	32.5	35.5	31.1	19.3	25.2	30.4	20.6	20.2	17.6	19.7	24.5	23.5	23.9	23.7
2 a.m.	32.7	30.3	26.6	25.6	17.3	17.8	26.8	17.3	16.2	14.7	19.9	23.0	20.0	22.7	21.1
4 a.m.	34.1	26.8	22.3	22.8	15.6	20.1	22.6	17.4	15.7	19.2	18.8	27.6	18.9	23.6	20.9
6 a.m.	32.4	23.3	23.4	35.4	26.2	23.6	26.9	32.9	27.1
8 a.m.	38.7	32.9	31.5	31.5	28.3	28.4	42.0	32.9	31.0	25.9	35.5	29.8	32.3	31.7	32.1
10 a.m.	45.0	36.2	34.1	43.7	30.4	32.8	43.5	32.5	33.4	32.8	38.3	41.0	35.6	36.8	36.0
Noon	50.0	41.7	43.6	44.6	27.3	28.1	40.3	31.8	32.9	26.7	45.4	39.6	34.0	38.0	35.5
2 p.m.	43.7	41.7	40.9	41.2	31.6	28.5	33.2	28.0	32.0	27.7	38.7	39.4	32.2	36.8	33.9
4 p.m.	43.7	42.2	37.1	36.8	31.9	28.0	39.7	31.6	32.4	29.8	37.8	42.4	33.8	37.1	35.0
6 p.m.	49.4	45.8	44.8	39.8	35.8	27.0	39.8	35.0	36.4	34.8	41.8	44.0	35.4	41.5	37.5
8 p.m.	46.0	43.5	40.0	38.3	33.2	35.8	44.4	38.1	35.5	30.7	43.7	44.2	37.4	39.4	38.0
10 p.m.	44.5	39.2	45.8	43.2	38.7	37.7	42.7	38.4	39.9	34.7	34.2	42.7	39.6	38.4	39.2
Means.....	38.8	36.2	35.2	35.3	27.4	27.6	36.4	28.7	28.5	26.2	31.8	35.0	30.3	32.4	31.1

TABLE XXXVII.—Mean electrical tension at each observation-hour, as deduced from all the positive observations *below* 60 div. in the years 1845, 1846 and 1847, arranged under the respective months, with the mean electrical tension of the summer, winter, and year.

Period.	January.	February.	March.	April.	May.	June.	July.	August.	September.	October.	November.	December.	Summer.	Winter.	Year.
Midnight...	23.0	27.1	30.2	26.6	20.6	21.2	22.1	20.0	20.1	20.6	22.5	28.1	21.3	24.5	22.6
2 a.m.	23.2	24.3	27.0	22.6	17.9	17.4	19.3	15.1	16.5	18.9	20.2	26.8	17.8	23.2	20.1
4 a.m.	23.5	25.7	22.6	21.1	16.6	20.2	19.0	15.7	15.7	20.0	19.3	24.6	17.8	22.7	19.9
6 a.m.	26.1	26.3	24.2	22.2	26.9	21.2	22.1	21.9	23.7	24.7	23.7
8 a.m.	33.6	31.9	33.3	33.1	30.3	25.7	30.9	25.3	27.6	27.3	32.6	30.2	28.5	31.2	29.6
10 a.m.	37.3	35.2	37.9	39.7	30.7	29.2	32.3	26.8	29.7	33.4	34.8	32.1	30.9	35.0	32.3
Noon	41.4	39.6	39.2	37.0	26.8	25.5	30.9	26.8	29.7	31.8	34.8	34.6	29.4	37.1	32.2
2 p.m.	36.0	42.2	38.1	36.3	28.3	25.2	29.4	28.0	30.7	32.2	35.9	36.8	29.5	36.3	32.0
4 p.m.	38.1	39.8	37.2	33.1	29.9	25.0	31.9	30.1	31.6	33.4	36.5	36.5	30.1	36.6	32.5
6 p.m.	38.7	41.3	38.1	36.9	32.2	27.0	33.7	32.1	32.2	33.7	39.6	40.3	32.1	38.0	34.1
8 p.m.	38.9	44.5	36.1	39.7	33.8	31.9	34.8	30.9	32.5	32.4	39.0	40.4	33.5	37.7	34.8
10 p.m.	33.0	37.1	38.2	32.3	35.5	35.2	36.9	34.3	34.3	35.3	37.2	36.5	35.0	36.2	35.5
Means.....	31.5	33.7	33.1	31.8	26.8	25.4	28.8	25.0	26.2	28.1	31.5	32.9	27.1	31.4	28.6

suggested the probability that the forenoon and evening maxima result more or less from the presence of aqueous vapour, either in an invisible or condensed state.

3. With a view to submit this notion to the test of observation, an attempt has been made (it must be confessed of a very rough and arbitrary character) to separate the high from the low tensions; the point 60 div. of Volta's electrometer No. 1 has been provisionally assumed as the separating point, and all tensions above it have been regarded as high, those below it the converse. The result of this separation, so far as the low tensions are concerned, has been to exhibit series of curves, those of the summer and entire year being somewhat in harmony with the aggregate curves for the same periods; the forenoon and evening maxima however are greatly subdued, but still the evening holds the most prominent position. The curves of the entire year suggest the probability that a single progression would be obtained on the removal of the two maxima.

4. The winter curves of low tension strongly confirm this suggestion. The approach to a single progression is very apparent in the winters of 1845 and 1847; the mean curve however still presents the two maxima, although their altitudes are considerably more equal in value than any of the curves yet contemplated; their interval in time (6 hours) is also less than most of the others, especially the aggregate curves, the most usual interval of these being 12 hours.

5. The salient points characterizing the two series of curves (aggregate and low tension) are a decided development of the forenoon and evening maxima in the aggregate, and a considerable subduing of these features with an approach to a single progression in the low.

Diurnal period above 60 div., Year.—We are now prepared to enter on the discussion of the high tensions, with the expectation that the two maxima so prominently developed in the aggregate curves will form very decided features in those deduced from observations above 60 div. It is necessary to observe here, that the observations above 60 div. will not furnish the entire diurnal march of the high tensions, none being recorded at the hours of midnight and 2 A.M.; very few indeed are entered at 4 A.M.; and those finding entrance at 6 A.M. being mostly confined to the summer half-year, the diurnal march cannot be accurately said to commence until 8 A.M. In the following tables and curves, with the exception of those having reference to the summer half-year, the diurnal march is given between 8 A.M. and 10 P.M. inclusive; in the summer it commences two hours earlier.

The 2647 high readings during the three years are thus divided among the twelve observation-hours:—

TABLE XXXVIII.

Number of positive readings above 60 div. at each observation-hour in the three years 1845, 1846 and 1847.

Year.	Mid.	2 a.m.	4 a.m.	6 a.m.	8 a.m.	10 a.m.	Noon.	2 p.m.	4 p.m.	6 p.m.	8 p.m.	10 p.m.	Sums.
1845.	3	18	92	103	73	75	87	107	130	164	852
1846.	2	20	118	124	76	83	86	110	138	157	914
1847.	3	26	124	126	72	73	80	96	129	152	881
Sums.	8	64	334	353	221	231	253	313	397	473	2647

In connection with this table it will be observed that it furnishes two

periods, each being marked by a greater number of readings than the intermediate period between them. Of these, the last, viz. that occurring at 6, 8 and 10 P.M., presents the greatest number of observations, and it is to be noticed, that both periods coincide with the epochs of the forenoon and evening maxima, as developed in the aggregate curves. This of itself indicates that the greatest number of high readings occur at those epochs, and that the maxima result more from a systematic occurrence of the high than the low readings. There is a difference between the greatest number, 473, at 10 P.M., and the smallest (excluding 4 and 6 A.M.), 221, at noon of 252.

TABLE XXXIX.

Mean electrical tension above 60 div. at each observation-hour in the three years 1845, 1846 and 1847, with the mean diurnal period as deduced from the whole.

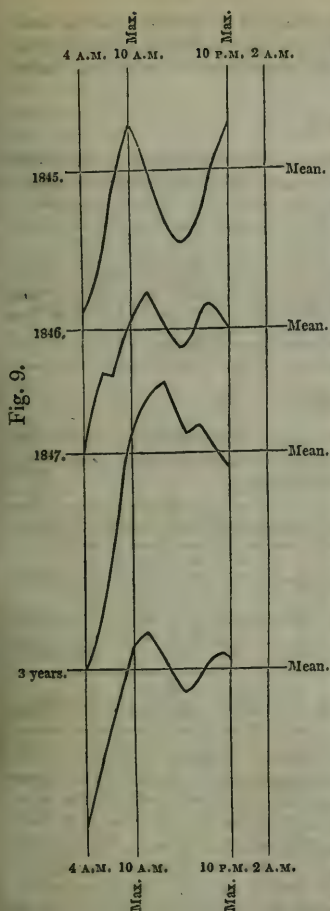
Year.	Mid.	2 a.m.	4 a.m.	6 a.m.	8 a.m.	10 a.m.	Noon.	2 p.m.	4 p.m.	6 p.m.	8 p.m.	10 p.m.	Mean.
			div.	div.	div.	div.	div.	div.	div.	div.	div.	div.	div.
1845.	85.0	116.7	167.7	205.6	173.7	144.3	130.2	146.0	187.9	205.8	173.2
1846.	72.5	123.9	122.2	153.2	176.3	147.6	137.2	163.9	162.1	146.2	149.7
1847.	70.8	110.0	164.7	219.6	244.9	249.2	215.5	222.8	204.4	191.6	205.6
Mean.	76.6	116.2	150.5	192.2	197.8	178.6	159.6	175.8	184.3	181.5	175.9

TABLE XL.

Excess or defect of the mean electrical tension above 60 div. at each observation-hour, as compared with the mean of the year for the three years 1845, 1846 and 1847, and the mean diurnal period.

Year.	Mid.	2 a.m.	4 a.m.	6 a.m.	8 a.m.	10 a.m.	Noon.	2 p.m.	4 p.m.	6 p.m.	8 p.m.	10 p.m.	Mean.
			div.	div.	div.	div.	div.	div.	div.	div.	div.	div.	div.
1845.	88.2	56.5	5.5	32.4	0.5	28.9	43.0	27.2	14.7	32.6	173.2
1846.	77.2	25.8	27.5	3.5	26.6	2.1	12.5	14.2	12.4	3.5	149.7
1847.	134.8	95.6	40.9	14.0	39.3	43.6	9.9	17.2	1.2	14.0	205.6
Mean.	99.3	59.7	25.4	16.3	21.9	2.7	16.3	0.1	8.4	5.6	175.9

Although the movements, as exhibited in the above tables, are decidedly *irregular*, yet the indications of a double progression are by no means deficient; they appear very prominently in the period for the year 1845. In this year the rise is very regular until 10 A.M., after which a fall, quite as regular, takes place between 10 A.M. and 4 P.M., and then the tension increases quite as regularly until 10 P.M. In 1846 and 1847 these movements are not so distinct, especially in the latter year, in which a great tendency to a single maximum about 2 P.M. occurs; there is however a subordinate maximum at 6 P.M. In 1846 the two maxima are developed, the forenoon being the principal. The mean curve of the three years exhibits a period of tolerable regularity, in which the two maxima are well-marked, that of the forenoon being the *highest*; the epochs are noon and 8 P.M.



Previous to examining the summer curves of high tension, it will be desirable to direct our attention to those of the winter; two circumstances contribute to this mode of proceeding. In discussing the curves of low tension, we found the greatest approach to a single progression occurring in the winter, and this would suggest that in the same season we ought to find the most decided development of the two maxima in the curves of high tension, which give to the aggregate curves the feature of a double progression. The great majority of readings during the summer being below 60 div., those above will be considerably less in number than the high readings in the winter, and it is consequently to be expected that the movements of the high tensions (simply considered as such) will be much more irregular in the summer than in the winter: in a word, if we can at all find any unequivocal indications of regularity of movement among the high tensions, we are much more likely to find them in the winter than in the summer.

Diurnal period above 60 div., Winter.—The entire number of high readings, 2647, is thus divided:—

Winter	1979
Summer	668
	<hr/> 2647

The following table exhibits the distribution of the winter observations over the twelve observation-hours:—

TABLE XLI.

Number of positive readings above 60 div. at each observation-hour in the three winters of 1845, 1846 and 1847.

Year.	Mid.	2 a.m.	4 a.m.	6 a.m.	8 a.m.	10 a.m.	Noon.	2 p.m.	4 p.m.	6 p.m.	8 p.m.	10 p.m.	Sums.
1845.....	2	66	84	64	68	74	83	88	96	625
1846.....	6	81	93	71	77	76	93	92	107	696
1847.....	3	80	91	66	67	70	83	94	104	658
Sums.....	11	227	268	201	212	220	259	274	307	1979

It will be seen from these numbers that the distribution of readings somewhat assimilates to that of the entire year, being more numerous about the

epochs of the forenoon and evening maxima. It is however much more equable, the difference between the greatest and least numbers, excluding the 11 at 6 A.M., being only 106. A proportionate regularity in the diurnal march may consequently be expected.

TABLE XLII.

Mean electrical tension above 60 div. at each observation-hour in the three winters of 1845, 1846 and 1847, with the mean diurnal period of winter.

Year.	Mid.	2 a.m.	4 a.m.	6 a.m.	8 a.m.	10 a.m.	Noon.	2 p.m.	4 p.m.	6 p.m.	8 p.m.	10 p.m.	Mean.
				div.	div.	div.	div.	div.	div.	div.	div.	div.	div.
1845.	85.0	184.8	231.1	188.0	146.6	139.6	167.0	223.1	259.1	196.0
1846.	83.3	130.4	163.2	171.9	145.0	136.9	174.5	198.7	164.7	161.1
1847.	165.0	206.0	248.0	261.1	257.1	235.0	244.8	247.4	221.8	238.7
Mean.	105.9	172.9	213.3	206.3	180.9	169.0	194.6	223.3	213.6	197.9

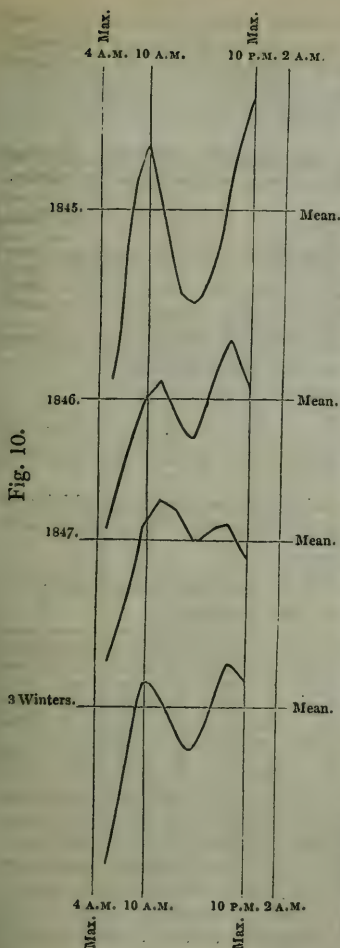
TABLE XLIII.

Excess or defect of the mean electrical tension above 60 div. at each observation-hour, as compared with the mean of each winter in the years 1845, 1846 and 1847, and the mean of the three winters.

Year.	Mid.	2 a.m.	4 a.m.	6 a.m.	8 a.m.	10 a.m.	Noon.	2 p.m.	4 p.m.	6 p.m.	8 p.m.	10 p.m.	Mean.
				—	—	+	—	—	—	—	+	+	
1845.	111.0	11.2	35.1	8.0	49.4	56.4	29.0	27.1	63.1	196.0
				—	—	+	+	—	—	+	+	+	
1846.	77.8	30.7	2.1	10.8	16.1	24.2	13.4	37.6	3.6	161.1
				—	—	+	+	+	—	+	+	—	
1847.	73.7	32.7	9.3	22.4	18.4	3.7	6.1	8.7	16.9	238.7
Mean.	92.0	25.0	15.4	8.4	17.0	28.9	3.3	25.4	15.7	197.9

There can be no question that a much greater regularity of movement characterizes these periods than we found appertaining to those of the entire year. In each of them we find the two maxima well-developed; in the winter of 1847 the forenoon maximum was the highest, but in other respects they agree more or less closely with the aggregate winter curves. The diurnal march is well-traced: commencing at 8 A.M., we find the forenoon maximum attained at 10 A.M., then a well-marked fall until 4 P.M., the afternoon minimum, after which a regular and rather rapid rise until 8 P.M., the epoch of the evening maximum, which is followed by a diminution of tension at 10 P.M. The annexed curves (fig. 10), which may be well compared with those on page 143, exhibit all the winter phenomena of high tension with considerable distinctness. It may be remarked, that in 1845 the evening maximum occurred at 10 P.M., and that a close agreement, in this respect, obtains between the high tension and aggregate curves in the winter of 1845.

In our remarks on the winter curves of aggregate tension (see page 123), we noticed the influence which the winter curves exerted on those of the entire year, and suggested the probability that the *higher* tensions materially influence the general results. This is very strikingly illustrated by the comparison of the winter curves of high tension with those of the same season as deduced from the aggregate observations; the main features of the curves in both series are similar, the principal difference consisting in the values of the



maxima in the winter of 1847. We see at a glance how greatly the forms of the aggregate curves *depend* on the *higher* tensions. On comparing the two series with those of the entire year (aggregate tension), the influence of the *high* tensions upon the whole is readily traced. We see the winter curves of high tension strongly influencing the winter curves of aggregate tension, and these again the aggregate of the entire year, the three series of curves closely resembling each other. The influence of the high tension entire year on the curves of aggregate tension for the same period is not so striking; the summer readings modify the curves, and illustrate the remarks we have already offered on the *variability* of the point of separation.

TABLE XLIV.

Comparison of the excess or defect from the mean of the diurnal periods of winter, as deduced from the aggregate observations and from those above 60 div.

Value.	Mid.	2 a.m.	4 a.m.	6 a.m.	8 a.m.	10 a.m.	Noon.	2 p.m.	4 p.m.	6 p.m.	8 p.m.	10 p.m.	Mean.
	div.	div.	div.	div.	div.	div.	div.	div.	div.	div.	div.	div.	div.
Aggregate...	77.6	78.9	79.4	55.6	9.0	27.9	13.7	3.9	0.6	27.3	55.2	43.4	102.1
Above 60 div.	92.0	25.0	15.4	8.4	17.0	28.9	3.3	25.4	15.7	197.9

In the above table the correspondence within certain limits as to excess and defect, in reference to the mean of each period, is well seen; also the striking development of the forenoon and evening maxima in each case. Upon the continuation of the observations of high tension at midnight, 2 and 4 A.M. in the winter, the mean line would be lowered and the correspondence rendered more complete.

During the day the movements do not very materially differ from those of the aggregate curves for the same periods; this is evident from the following table:—

TABLE XLV.

Synopsis of the principal points in the aggregate and high tension winter curves.

Value.	Forenoon Maximum above Minimum.	Evening Maximum above Minimum.	Evening Maximum above Forenoon.
Aggregate	div. 28·5	div. 55·8	div. 27·3
Above 60 div.....	44·3	54·3	10·0

Diurnal period above 60 div., Summer.—The 668 readings upon which this period is based are thus distributed among the twelve observation-hours:—

TABLE XLVI.

Number of positive readings above 60 div. at each observation-hour in the three summers of 1845, 1846 and 1847.

Year.	Mid.	2 a.m.	4 a.m.	6 a.m.	8 a.m.	10 a.m.	Noon.	2 p.m.	4 p.m.	6 p.m.	8 p.m.	10 p.m.	Sums.
1845.....	3	16	26	19	9	7	13	24	42	68	227
1846.....	2	14	37	31	5	6	10	17	46	50	218
1847.....	3	23	44	35	6	6	10	13	35	48	223
Sums.....	8	53	107	85	20	19	33	54	123	166	668

It will be at once apparent that these readings are but unequally distributed. As in the former instances, the greatest numbers occur about the hours of the forenoon and evening maxima; but the numbers about noon and 2 P.M. are so small as to render it questionable whether we should regard the periods deduced from the observations as true representatives of natural phenomena: we shall however give them in the same form as the others, and in our remarks solicit particular attention to the maxima occurring in each summer, either at noon or 2 P.M.

TABLE XLVII.

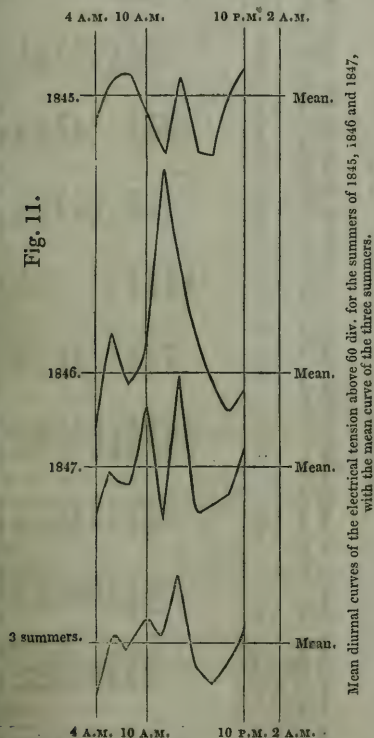
Mean electrical tension above 60 div. at each observation-hour in the three summers of 1845, 1846 and 1847, with the mean diurnal period of summer.

Year.	Mid.	2 a.m.	4 a.m.	6 a.m.	8 a.m.	10 a.m.	Noon.	2 p.m.	4 p.m.	6 p.m.	8 p.m.	10 p.m.	Mean.
1845.....	div. 85·0	div. 120·7	div. 124·3	div. 92·8	div. 72·2	div. 122·1	div. 76·3	div. 73·1	div. 114·0	div. 130·6	div. 110·6
1846.....	72·5	141·2	104·3	123·3	238·0	181·2	139·7	105·9	88·7	106·6	113·3
1847.....	70·8	102·8	89·7	145·8	66·7	161·2	79·5	82·3	88·9	126·0	107·7
Mean.....	76·6	118·4	103·1	125·7	112·0	153·2	96·6	85·6	97·4	122·1	110·5

TABLE XLVIII.

Excess or defect of the mean electrical tension above 60 div. at each observation-hour, as compared with the mean of each summer in the years 1845, 1846 and 1847, and the mean of the three summers.

Year.	Mid.	2 a.m.	4 a.m.	6 a.m.	8 a.m.	10 a.m.	Noon.	2 p.m.	4 p.m.	6 p.m.	8 p.m.	10 p.m.	Mean.
			div.	div.	div.	div.	div.	div.	div.	div.	div.	div.	div.
1845.	25·6	10·1	13·7	17·8	38·4	11·5	34·1	37·5	3·4	20·0	110·6
1846.	40·8	27·9	9·0	10·0	124·7	67·9	26·4	7·4	24·6	6·7	113·3
1847.	36·9	4·9	18·0	38·1	41·0	53·5	28·2	25·4	18·8	18·3	107·7
Mean.	33·9	7·9	7·4	15·2	1·5	42·7	13·9	24·9	13·1	11·6	110·5



In the annexed curves (fig. 11), the general irregularity which is apparent in the tables is very distinctly marked. We have already alluded to the maxima at 2 P.M. or noon; with one exception they are the highest of each curve; but how far, from the small number of observations that contribute to their determination, they can be regarded as truly representing a mean increase of the electrical tension above 60 div. at this period of the day, must, we apprehend, be left for future observations to determine. It is however likely that even on a long series of years the number of high tensions at noon and 2 P.M. will always bear a very small proportion to those at other hours, especially near the epochs of the forenoon and evening maxima. In two of the aggregate summer curves, 1845 and 1847, we have small subordinate maxima at 2 P.M., which, when compared with the two principal, are scarcely apparent. Nothing of the kind appears in the winter curves, either aggregate or high tension, so that if the maximum about 2 P.M. in summer

truly represent a natural phenomenon, it is one peculiar to the summer months. The close approximation of the values of the means of each summer is an interesting feature, which suggests considerable hesitation in deciding on the character of these irregular curves. The aggregate and low tension summer curves also agree in their means, differing but little from each other in value.

TABLE XLIX.—Mean electrical tension at each observation-hour, as deduced from all the positive observations *above* 60 div. in the year 1845, arranged under the respective months, with the mean electrical tension of the summer, winter and year.

Period.	January.	February.	March.	April.	May.	June.	July.	August.	September.	October.	November.	December.	Summer.	Winter.	Year.
Midnight...															
2 A.M.					90.0	95.0	70.0						85.0		85.0
4 A.M.				97.5	191.2	71.8	200.0		82.5				120.7	85.0	116.7
6 A.M.				110.8	190.0	109.4		71.7	137.1	121.1	111.7	196.2	124.3	184.8	167.7
8 A.M.	293.4	276.9	77.2	114.7	63.3	96.2	60.0	80.0	69.2	81.4	186.6	239.5	92.8	231.1	205.6
10 A.M. ...	260.7	344.2	124.7		65.0	65.0		63.7	77.5	71.7	97.5	167.7	72.2	188.0	173.7
Noon	283.5	268.0	70.0	86.2	200.0			65.0	130.0	68.2	103.9	92.5	122.1	146.6	144.3
2 P.M.	273.2	211.4	71.6	65.0					73.3	69.2	154.5	145.0	76.5	139.6	130.2
4 P.M.	179.4	166.0	91.5	66.9	84.6								73.1	167.0	146.0
6 P.M.	226.0	229.6	86.9	73.2	70.8	62.5	67.5	63.3	85.4	88.2	134.8	160.4	73.1	167.0	146.0
8 P.M.	320.0	366.3	177.9	185.4	70.7	85.0	62.5	90.0	99.2	104.5	126.3	160.7	114.0	223.1	187.9
10 P.M. ...	232.4	546.8	217.2	203.1	107.8	73.3	125.8	132.2	87.9	138.2	169.2	188.4	130.6	259.1	205.8
Means.....	258.5	303.5	127.1	138.8	110.8	82.1	98.3	94.4	95.3	98.5	138.3	167.1	110.6	196.0	173.2

TABLE L.—Mean electrical tension at each observation-hour, as deduced from all the positive observations *above* 60 div. in the year 1846, arranged under the respective months, with the mean electrical tension of the summer, winter and year.

Period.	January.	February.	March.	April.	May.	June.	July.	August.	September.	October.	November.	December.	Summer.	Winter.	Year.
Midnight...															
2 A.M.					75.0		70.0						72.5		72.5
4 A.M.					385.8	85.0	80.0		75.0				141.2	83.3	123.9
6 A.M.				72.5	113.2	77.1	80.0	88.7	72.5	101.0	111.8		104.3	130.4	122.2
8 A.M.	207.1	136.0	142.9	129.8	102.5	121.7	64.2	82.5	76.7	135.3	124.6	178.1	123.3	163.2	153.2
10 A.M. ...	179.6	166.8	157.7	161.2	500.0					328.9	129.2	216.1	238.0	171.9	176.3
Noon	141.0	106.2	109.2	63.3	70.0					164.0	80.2	255.4	181.2	145.0	147.6
2 P.M.	152.3	92.1	79.5	70.0	400.0			182.5			77.3	229.0	139.7	136.9	137.2
4 P.M.	127.5	119.4	82.0	208.0	75.0	90.0		67.5	62.5	115.6	79.1	301.9	105.9	171.5	163.9
6 P.M.	196.0	195.8	91.9	149.2	65.0	132.5		80.0	70.0	115.8	80.2	244.6	88.7	198.7	162.1
8 P.M.	196.4	213.7	247.0	110.6	73.4	85.0	69.6	81.2	75.5	121.1	74.3	181.7	106.6	164.7	146.2
10 P.M. ...	160.1	201.6	132.6	130.1	107.7	85.8	71.2	72.5	102.5	219.0					
Means.....	171.5	160.0	132.7	127.7	145.3	94.6	71.3	97.6	78.4	155.7	92.0	211.0	113.3	161.1	149.7

TABLE LI.—Mean electrical tension at each observation-hour, as deduced from all the positive observations *above* 60 div. in the year 1847, arranged under the respective months, with the mean electrical tension of the summer, winter and year.

Period.	January.	February.	March.	April.	May.	June.	July.	August.	September.	October.	November.	December.	Summer.	Winter.	Year.
Midnight...	63.7	85.0	70.8	70.8
2 A.M.	81.2	140.5	72.5	80.0	102.8	165.0	110.0
4 A.M.	60.0	91.7	76.0	66.1	166.9	93.0	89.7	206.0	164.7
6 A.M.	277.6	213.6	214.4	104.9	82.5	316.2	71.0	72.9	177.1	177.7	187.1	145.8	248.0	219.6
8 A.M.	365.0	293.6	148.2	89.4	67.5	65.0	70.0	140.0	195.8	119.1	66.7	261.1	244.9
10 A.M. ...	354.0	353.8	112.1	70.0	60.0	400.0	65.0	65.0	157.7	140.2	161.2	257.1	249.2
Noon	388.5	372.0	67.9	183.7	67.5	200.0	75.0	66.7	68.0	124.8	131.2	79.5	235.0	215.5
2 P.M.	442.2	271.8	78.7	65.0	63.3	135.0	90.0	70.0	80.0	218.6	165.3	82.3	244.8	222.8
4 P.M.	390.7	290.4	121.4	66.0	100.0	70.7	75.0	76.4	105.6	152.7	204.3	88.9	247.4	204.4
6 P.M.	393.3	378.9	140.1	80.4	162.5	320.8	65.6	91.7	101.4	143.1	211.2	126.0	221.8	191.6
8 P.M.	311.6	288.7	155.5	83.3	75.8	66.2
10 P.M.
Means.....	364.2	305.5	146.3	91.6	91.2	68.3	194.8	72.5	76.2	124.4	168.2	156.9	107.7	238.7	205.6

TABLE LII.—Mean electrical tension at each observation-hour, as deduced from all the positive observations *above* 60 div. in the years 1845, 1846 and 1847, arranged under the respective months, with the mean electrical tension of the summer, winter and year.

Period.	January.	February.	March.	April.	May.	June.	July.	August.	September.	October.	November.	December.	Summer.	Winter.	Year.
Midnight...
2 A.M.	75.0	76.6	76.6
4 A.M.	74.2	140.4	72.5	80.0	100.0	118.4	105.9	116.2
6 A.M.	106.5	91.8	248.4	77.2	88.7	77.2	92.6	127.7	127.5	115.4	103.1	172.9	150.5
8 A.M.	259.2	210.3	156.9	114.7	125.5	86.5	243.9	77.2	72.9	142.7	168.9	189.0	125.7	213.3	192.2
10 A.M. ...	277.8	272.4	146.0	126.8	79.1	115.3	65.0	65.8	77.5	200.8	139.3	179.7	112.0	206.3	197.8
Noon	286.5	250.6	100.8	71.8	281.2	65.0	400.0	135.5	130.0	97.3	112.0	175.5	153.2	180.9	178.6
2 P.M.	289.9	234.1	74.5	106.2	187.0	200.0	71.2	68.1	85.1	115.2	177.8	96.6	169.0	159.6
4 P.M.	281.0	191.9	85.3	130.7	77.2	90.0	94.5	73.3	76.2	99.0	149.2	225.5	85.6	194.6	175.8
6 P.M.	282.9	242.2	98.3	96.5	73.7	97.5	68.8	84.7	56.6	110.1	122.4	212.7	97.4	223.3	184.3
8 P.M.	309.0	324.8	187.7	122.6	94.7	85.0	193.8	102.5	91.8	154.4	133.7	191.3	122.1	213.6	181.5
10 P.M. ...	236.3	339.7	164.9	137.7	104.7	76.2
Means.....	277.1	262.8	136.0	118.6	118.9	85.9	146.1	87.6	84.8	125.0	133.7	184.8	110.5	197.9	175.9

In addition to the principal results of the discussion of the aggregate and low tensions on pages 138 and 141, we find from that of the high, that the movements of the electrical tension above 60 div. in the *winter* are such as strongly to confirm the suggestion of the forenoon and evening maxima resulting from such high tensions.

Tables XLIX., L. and LI. exhibit the mean electrical tension above 60 div. at each observation-hour for each month in the three years 1845, 1846 and 1847, with the monthly, seasonal and yearly means.—The characters of the monthly movements are exhibited to the eye in the sheets of curves illustrating this report. See Plate X. and XI.

Table LII. exhibits the mean monthly electrical tension at each observation-hour, deduced from the observations of *three* months; also the mean summer, winter and yearly tensions, deduced from the observations of *three* summers, winters and years. The last line in the table, to which the word "Means" is prefixed, exhibits the mean tension in each month, as deduced from all the separate monthly observations; *i. e.* the mean tension of January, 277.1 div., is the result of all the January observations in the three years. The same thing holds good of the seasonal and yearly mean tensions.

The curves projected from these numbers will be found on Plate XI.

ANNUAL PERIOD.

Aggregate observations.—One of the principal results of the foregoing discussion has been to exhibit the march of the electrical tension during the twenty-four hours constituting the period of a day. This march has been found to present two well-defined maxima, in most instances removed from each other by an interval of twelve hours, the principal occurring at 10 P.M. and the inferior at 10 A.M. Two minima have also been ascertained, the principal at 4 A.M. and the subordinate at 4 P.M. At a particular season of the year, there have been indications of a *curve of low tension presenting considerable approximation to a single progression, more or less in harmony with the curve of temperature*; but the curve deduced from all the positive observations is not in harmony with the curve of temperature, inasmuch as neither of the maxima corresponds with either of its turning-points. We must not however forget, that the greatest development of electricity, so far as the diurnal period is concerned, takes place from sunrise to 10 P.M., and includes the period that the sun is above the horizon, and to this extent there is a connection between the temperature and the electrical tension. We now proceed to examine those changes of the electrical tension, the period of which is completed in the same time that the earth is occupied in making a revolution round the sun.

The following table contains the number of readings in each month of the three years which form the bases on which the results in the succeeding tables rest. It will be remarked, that the greatest number occur in the summer and the least in winter, the cause of which has been already referred to as resulting from the cessation of observations at 6 A.M. during the winter months.

TABLE LIII.

Number of positive readings in each month of the three years 1845, 1846 and 1847.

Year.	Jan.	Feb.	Mar.	April.	May.	June.	July.	Aug.	Sept.	Oct.	Nov.	Dec.	Sums.
1845.	287	258	228	280	305	299	330	313	318	287	220	249	3374
1846.	264	228	276	259	308	308	327	314	316	269	280	250	3399
1847.	244	226	278	271	313	300	320	315	318	298	265	255	3403
Sums.	795	712	782	810	926	907	977	942	952	854	765	754	10176

TABLE LIV.

Mean electrical tension of each month in the three years 1845, 1846 and 1847, with the mean annual period, as deduced from the whole.

Year.	Jan.	Feb.	Mar.	April.	May.	June.	July.	Aug.	Sept.	Oct.	Nov.	Dec.	Mean.
	div.	div.	div.	div.	div.	div.	div.	div.	div.	div.	div.	div.	div.
1845.	109.3	190.3	64.5	56.1	38.7	26.0	25.9	29.9	37.5	46.2	83.9	84.2	63.1
1846.	95.9	100.1	78.9	63.7	42.8	33.0	31.3	26.3	27.2	65.6	49.8	160.3	61.3
1847.	258.8	206.6	79.6	52.2	32.3	28.8	59.7	31.9	34.3	41.0	78.7	84.3	76.3
Mean.	150.7	166.6	75.0	57.2	37.9	29.3	38.8	29.4	33.0	50.5	69.6	109.5	66.9

TABLE LV.

Excess or defect of the mean electrical tension of each month, as compared with the mean of the year for the three years 1845, 1846 and 1847, and the mean annual period.

Year.	Jan.	Feb.	Mar.	April.	May.	June.	July.	Aug.	Sept.	Oct.	Nov.	Dec.	Mean.
	div.	div.	div.	div.	div.	div.	div.	div.	div.	div.	div.	div.	div.
1845.	46.2	127.2	1.4	7.0	24.4	37.1	37.2	33.2	25.6	16.9	20.8	21.1	63.1
1846.	34.6	38.8	17.6	2.4	18.5	28.3	30.0	35.0	34.1	4.3	11.5	99.0	61.3
1847.	182.5	130.3	3.3	24.1	44.0	47.5	16.6	44.4	42.0	35.3	2.4	8.0	76.3
Mean.	83.8	99.7	8.1	9.7	29.0	37.6	28.1	37.5	33.9	16.4	2.7	42.6	66.9

An annual period in the electrical tension is not only very perceptible, but unquestionable. It is, with an exception hereafter to be noticed, a single progression having its turning-points in February and June. The exception alluded to consists in an increase of tension in July; but as this occurred only in one year (1847), it will form the subject of remark further on. From the mean of the three years, we find that June and August present nearly the same electrical tension, the difference being only 0.1 div. In September a small rise occurs which is increased in October; the augmentation becomes more rapid from November to January and then receives a check, the February increment being less than those of December and January. In February the maximum is attained, which is succeeded in March by a very rapid diminution of tension which continues through April and May, the decrements becoming less in value until June, the month presenting the lowest tension.

From this progression those of individual years differ to a greater or less

extent: the turning-points do not occur in each year in the same months, and the range of tension differs materially:

The year 1847, as we have already noticed, presents the highest tensions; this is very apparent from the following table of range.

TABLE LVI.

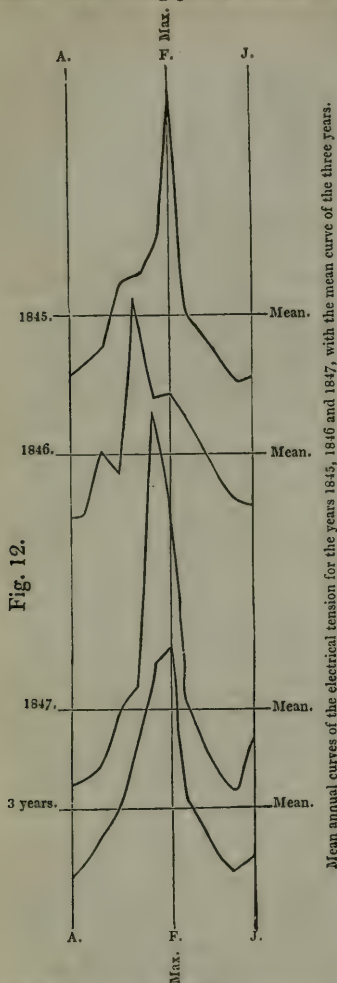
Mean annual range of the electrical tension in the years 1845, 1846 and 1847, with the mean annual range of the three.

Year.	Range.
	div.
1845.	164·4
1846.	134·0
1847.	230·0
Mean.	137·3

The greater development of electricity in the year 1847 occurred in the month of January. The annexed curves (fig. 12) are projected on precisely the same scale as those of the diurnal periods, and are strictly comparable with them.

On contrasting the annual with the diurnal period, we find a marked difference which is not of an ordinary character. In the diurnal period we found an increase of tension towards the forenoon, succeeded by a diminution, the tension still continuing *high* in comparison with readings obtained after 10 P.M., at which hour the highest tension was most usually observed. The periods characterized by high and low tensions were those at which the sun was above and below the horizon (speaking in a general sense), the development of electricity appearing to be connected with the *increase* of tem-

perature. In the annual period the reverse of this takes place: that portion of the year during which the sun is further removed from the northern temperate zone is characterized by the exhibition of electricity of much higher tension than that which is observed during the period when he is nearest thereto. From the months in which the greatest and least tensions occur, it appears that there is a connexion between the annual curve of temperature and that of the electrical tension, the progression of the latter being to a certain extent in harmony with that of the former, but inverse. It is well known that the same characteristic is presented by the annual curve of humidity, which is in inverse harmony with the annual curve of temperature, and this at once



directly connects the annual period of the electrical tension with that of the humidity, and strongly confirms the suggestion already offered, that the high tensions at least measure the electrical tension of aqueous vapour. In order to illustrate this, the mean annual period of humidity, deduced from five years' observations at the Royal Observatory, Greenwich, is placed in connexion with the annual period of the electrical tension in the following table, in which the electrical tension is expressed in *entire* divisions of Volta's electrometer No. 1, and the humidity in the natural scale, in which complete saturation is reckoned as equal to 1000.

TABLE LVII.

Mean annual periods of electrical tension and humidity.

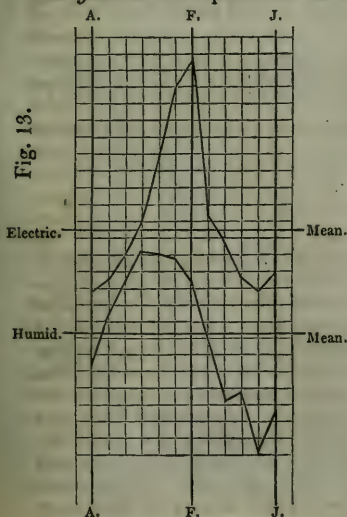
Period.	Jan.	Feb.	Mar.	April.	May.	June.	July.	Aug.	Sept.	Oct.	Nov.	Dec.	Mean.
Electric..	151	167	75	57	38	29	39	29	33	51	70	109	67
Humid...	908	894	856	821	829	791	816	845	874	893	911	910	863

TABLE LVIII.

Comparison of the excess or defect from the mean of the electric and humid annual periods.

Period.	Jan.	Feb.	Mar.	April.	May.	June.	July.	Aug.	Sept.	Oct.	Nov.	Dec.	Mean.
Electric..	+ 84	+ 100	+ 8	- 10	- 29	- 38	- 28	- 38	- 34	- 16	+ 3	+ 42	67
Humid...	+ 45	+ 31	- 7	- 42	- 34	- 72	- 47	- 18	+ 11	+ 30	+ 48	+ 47	863

The *general* correspondence as to the months exhibiting the greatest degree of humidity and the greatest electrical tension is very perceptible. It is however to be remarked, that the maximum of electrical tension does not occur in the same month as that of humidity. In Table LVIII., the later occurrence of the turning-points of the annual period of electricity as compared with that of humidity is very striking.



In the annexed curves (fig. 13), the points in which these periods correspond as well as those in which they differ are rendered very apparent to the eye. The curve of humidity is projected on a scale suitable for comparing it with that of the electrical tension, 100 divisions of the natural scale before mentioned, or one-tenth of the whole, being considered as equal to one inch.

Low tension.—In the following table, which exhibits the distribution of low readings in each month of the three years, the greater number during the summer is very apparent; it will be remarked that July presents the greatest number and February the least; the proportion is nearly as 3 to 1.

TABLE LIX.

Number of positive readings below 60 div. in each month of the three years 1845, 1846 and 1847.

Year.	Jan.	Feb.	Mar.	April.	May.	June.	July.	Aug.	Sept.	Oct.	Nov.	Dec.	Sums.
1845.	184	107	145	211	257	277	315	287	271	206	113	149	2522
1846.	146	109	152	172	268	280	306	297	291	198	193	73	2485
1847.	79	83	167	190	289	291	273	292	279	253	174	152	2522
Sums.	409	299	464	573	814	848	894	876	841	657	480	374	7529

TABLE LX.

Mean electrical tension below 60 div. of each month in the three years 1845, 1846 and 1847, with the mean annual period, as deduced from all the positive readings below 60 div.

Year.	Jan.	Feb.	Mar.	April.	May.	June.	July.	Aug.	Sept.	Oct.	Nov.	Dec.	Mean.
	div.	div.	div.	div.	div.	div.	div.	div.	div.	div.	div.	div.	div.
1845.	25·8	30·7	28·7	29·0	25·2	21·5	22·5	24·0	27·5	25·6	32·4	28·6	25·9
1846.	34·7	34·6	35·1	31·3	27·5	26·8	28·5	22·3	22·8	33·3	30·7	37·3	28·8
1847.	38·8	36·2	35·2	35·3	27·4	27·6	36·4	28·7	28·5	26·2	31·8	35·0	31·1
Mean.	31·5	33·7	33·1	31·8	26·8	25·4	28·8	25·0	26·2	28·1	31·5	32·9	28·6

TABLE LXI.

Excess or defect of the mean electrical tension below 60 div. of each month, as compared with the mean of the years for the three years 1845, 1846 and 1847, and the mean annual period.

Year.	Jan.	Feb.	Mar.	April.	May.	June.	July.	Aug.	Sept.	Oct.	Nov.	Dec.	Mean.
1845.	—	+	+	+	—	—	—	—	+	—	+	+	25·9
	0·1	4·8	2·8	3·1	0·7	4·4	3·4	1·9	1·6	0·3	6·5	2·7	
1846.	+	+	+	+	—	—	—	—	—	+	+	+	28·8
	5·9	5·8	6·3	2·5	1·3	2·0	0·3	6·5	6·0	4·5	1·9	8·5	
1847.	+	+	+	+	—	—	—	—	—	+	+	+	
	7·7	5·1	4·1	4·2	3·7	3·5	5·3	2·4	2·6	4·9	0·7	3·9	31·1
Mean.	+	+	+	+	—	—	+	—	—	—	+	+	28·6
	2·9	5·1	4·5	3·2	1·8	3·2	0·2	3·6	2·4	0·5	2·9	4·3	

In the above tables we see an annual period nearly similar to that deduced from the entire series of positive observations during the three years. The main feature—that of an increase of electrical tension in the winter and a decrease in the summer—is the same in both periods; and from this circumstance the legitimate inference is, that the low tensions are affected by the presence of aqueous vapour as well as the high; consequently the arbitrary division at 60 div. fails at all seasons entirely to separate the electricity of aqueous vapour from that of the atmosphere, supposing the true march of the latter to be in harmony with that of the temperature. There are some minor differences between the two periods which it may be interesting to

notice here. The progression is not single; it presents a depression at or near the period of the maximum, and an elevation at or near the period of the minimum.

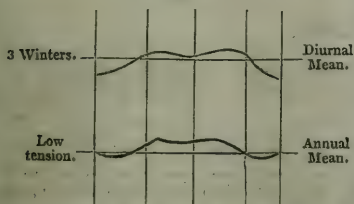
The maximum occurs in February and the minimum in August: commencing with the latter month, we have a gradual and uninterrupted rise until December of 7.9 div.; this is succeeded by a depression of 1.4 div. in January, and in February the maximum occurs, showing an increase on January of 2.2 div. The fall is then very gradual and uninterrupted until June—value 8.3 div. The elevation before spoken of occurs in July; it is as much as 3.4 div., and is succeeded by the minimum of August. The annual periods of single years partake of the same irregularity of movement which characterizes the annual periods deduced from all the positive observations.

The symmetrical position of the elevation and depression interrupting the general march of the electrical tension at July and January, and their coincidence with the usual turning-points of the annual curve of temperature, suggest the idea that they may be more or less connected with that curve; *i. e.* it is not improbable that they may be the turning-points of the annual curve which depicts the annual progression

of atmospheric electricity as distinguished from that of aqueous vapour, the latter being more strongly developed and consequently overpowering the former.

We have among the diurnal curves one that presents a striking similarity to that now under consideration; it is the curve of low tension for the mean

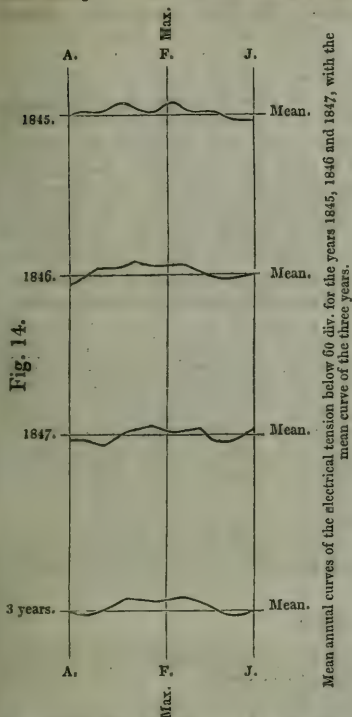
Fig. 15.



of the three winters. In our examination of this curve, we considered that its peculiar form arose from the tendency in the readings to exhibit a single progression which was interrupted by the presence of aqueous vapour affecting the lower readings. It will be observed that the two curves (fig. 15), although to a great extent possessing *similarity* of form, are strikingly

in contrast; they are to a great extent the converse of each other. In the winter we find the lower tensions struggling to maintain a single progression, which is overpowered, not by the maximum being depressed, but by the superposition of two maxima in all probability the effects of aqueous vapour,

Fig. 14.



and it is only after we have examined the yearly and summer curves that we find a tendency to a single progression in the winter. In the annual curve it is the aqueous vapour that produces the single progression; this is very apparent in the aggregate curves. When however we remove the tensions that appear more immediately to result from the presence of aqueous vapour, this single progression is interrupted at those points at which it is probable the influence of the vapour may be less than that of atmospheric electricity, and at these points only we have a corresponding elevation and depression. From the above considerations, both with regard to the diurnal and annual periods, we apprehend that it must be concluded, that a mere arbitrary division of the readings at any particular point will fail effectually to separate the electrical tension into its constituents, viz. that which is dependent on the solar action from that which results from the presence of aqueous vapour: nevertheless it appears, we apprehend, highly probable that the indications of a diurnal as well as an annual progression of atmospheric electricity, each having an ascending and descending branch, and consequently both being single progressions, are by no means of an uncertain character, and that the only requisite is a suitable mode of observation in order to apply *formulae* capable of effecting such a separation, whereby all electrical tensions resulting directly from the presence of aqueous vapour may be ascertained and deducted from aggregate tensions as measured by the electrometers; that the curves both of atmospheric electricity and the electrical tension of aqueous vapour may be exhibited each freed from the influence of the other, so that their connexion or non-connexion with other meteorological elements may be readily ascertained.

High tension.—In the following table the great difference between the high readings in summer and winter is very apparent: February furnishes the greatest number (413) and June the least (59); the proportion is exactly 7 to 1.

TABLE LXII.

Number of positive readings above 60 div. in each month of the three years 1845, 1846 and 1847.

Year.	Jan.	Feb.	Mar.	April.	May.	June.	July.	Aug.	Sept.	Oct.	Nov.	Dec.	Sums.
1845.	103	151	83	69	48	22	15	26	47	81	107	100	852
1846.	118	119	124	87	40	28	21	17	25	71	87	177	914
1847.	165	143	111	81	24	9	47	23	39	45	91	103	881
Sums.	386	413	318	237	112	59	83	66	111	197	285	380	2647

TABLE LXIII.

Mean electrical tension above 60 div. of each month in the three years 1845, 1846 and 1847, with the mean annual period, as deduced from all the positive readings above 60 div.

Year.	Jan.	Feb.	Mar.	April.	May.	June.	July.	Aug.	Sept.	Oct.	Nov.	Dec.	Mean.
1845.	258.5	303.5	127.1	138.8	110.8	82.1	98.3	94.4	95.3	98.5	138.3	167.1	173.2
1846.	171.5	160.0	132.7	127.7	145.3	94.6	71.3	97.6	78.4	155.7	92.0	211.0	149.7
1847.	364.2	305.5	146.3	91.6	91.2	68.3	194.8	72.5	76.2	124.4	168.2	156.9	205.6
Mean.	277.1	262.8	136.0	118.6	118.9	85.9	146.1	87.6	84.8	125.0	133.7	184.8	175.9

In the Tables LXII. LXIII. and LXIV. the annual period is very apparent, but it exhibits a much greater irregularity of movement, both in the individual years and in the mean of the three, than the annual period as deduced from all the observations. This irregularity of movement is well seen in the annexed curves (fig. 16), as well as the character which the high readings impart to the aggregate curves; for on comparing these with the aggregate curves on page 152, it will be observed that the latter present all the essential features of the curves of high tension, but so subdued that the movements appear more gentle and regular. In fact, throughout the series (excepting the summer months) the curves of high tension materially influence those as deduced from all the observations, and lead to the conclusion, that either throughout the year or during the winter, upon the supposition of high readings more directly measuring the electrical tension of aqueous vapour, the presence of such vapour materially affects the results. The same thing holds good with regard to the summer curves; for although the curves of high tension in the summer are very anomalous, yet the difference between the summer and winter curves of low tension, and the greater similarity between the aggregate and low tension summer curves, combined with the dissimilarity between the aggregate and low tension winter curves, strongly suggest that the summer low tension, as well as the aggregate curves, are materially influenced by the vapour, from the effects of which, as before observed, it is desirable the curves exhibiting the diurnal and annual march of electricity should be freed.

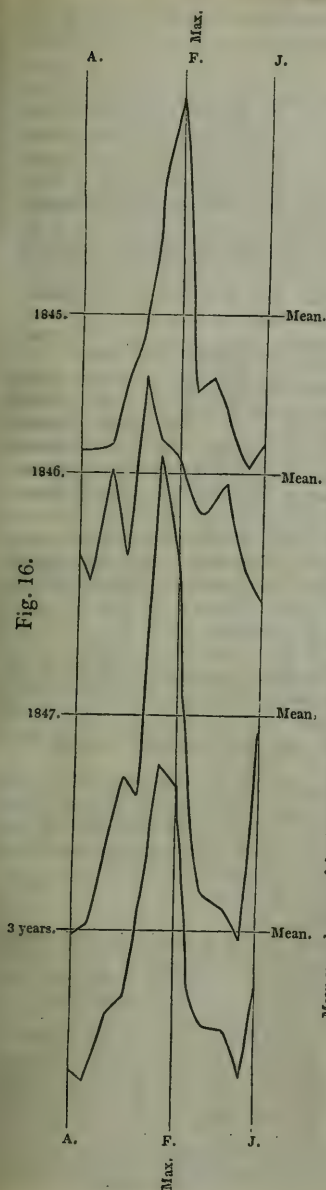


TABLE LXIV.

Excess or defect of the mean electrical tension above 60 div. of each month, as compared with the mean of the year for the three years 1845, 1846 and 1847, and the mean annual period.

Year.	Jan.	Feb.	Mar.	April.	May.	June.	July.	Aug.	Sept.	Oct.	Nov.	Dec.	Mean.
	div.	div.	div.	div.	div.	div.	div.	div.	div.	div.	div.	div.	div.
1845.	85.3	130.3	46.1	34.4	62.4	91.1	74.9	78.8	77.9	74.7	34.9	6.1	173.2
	+	+	—	—	—	—	—	—	—	+	—	+	
1846.	21.8	10.3	17.0	22.0	4.4	55.1	78.4	52.1	71.3	6.0	57.7	61.3	149.7
	+	+	—	—	—	—	—	—	—	—	—	—	
1847.	158.6	99.9	59.3	114.0	114.4	137.3	10.8	133.1	129.4	81.2	37.4	48.7	205.6
	+	+	—	—	—	—	—	—	—	—	—	+	
Mean.	101.2	86.9	39.9	57.3	57.0	90.0	29.8	88.3	91.1	50.9	42.2	8.9	175.9

SECTION 2.—*Discussion of Observations at Sunrise and Sunset.*

The observations made at sunrise and sunset furnish two series from which an interesting comparison with the mean annual period as deduced from three years' observations (see p. 151) may be derived. The epochs of observation of course are variable, coinciding in the summer with those points of the diurnal curve that are situated nearer the two *superior* turning-points, the principal extremes; while as regards the epochs of winter, that of sunrise approaches within two hours of the forenoon maximum, and that of sunset nearly coincides with the afternoon minimum. That this variability influences, no doubt to a considerable extent, the exhibition of electrical tension at the epochs of sunrise and sunset, there can be no question. Upon consulting Table V. (p. 118) it will be seen that the sunrise observations throughout the year, with the exception of those just about midwinter, fall in that portion of the diurnal curve that is *below* the mean of the whole year; while those at sunset appertain to a portion of the curve *above* the mean. We are therefore prepared to expect that the sunset observations throughout the year should present higher electrical tensions than those at sunrise, and such is the general fact—the tension at sunset is with but few exceptions higher than that at sunrise.

The entire number of observations employed in the deduction of the following results is 3367; of these, 1712 were made at sunrise and 1655 at sunset. The following tables exhibit the distribution of these observations over the respective months of the five years during which they were made, and also the mean of each month as based upon these numbers.

TABLE LXV.

Number of positive readings of the electric tension at sunrise in each month from August 1843 to July 1848 inclusive.

Year.	Jan.	Feb.	Mar.	April.	May.	June.	July.	Aug.	Sept.	Oct.	Nov.	Dec.	Sums.
1843.	26	30	24	27	31	138
1844.	28	28	27	30	31	30	30	28	24	28	28	31	343
1845.	26	27	23	29	31	24	27	28	30	30	30	31	336
1846.	30	27	30	26	31	26	27	30	29	27	30	30	343
1847.	31	27	31	28	30	24	29	29	30	31	29	28	347
1848.	31	29	28	30	31	28	28	205
Sums.	146	138	139	143	154	132	141	141	143	140	144	151	1712

TABLE LXVI.

Mean electric tension at sunrise in each month from August 1843 to July 1848 inclusive, with the mean monthly electric tension deduced from them.

Year.	Jan.	Feb.	Mar.	April.	May.	June.	July.	Aug.	Sept.	Oct.	Nov.	Dec.	Mean.
	div.	div.	div.	div.	div.	div.	div.	div.	div.	div.	div.	div.	div.
1843.	15.7	26.1	17.4	105.0	40.7	
1844.	127.0	93.9	23.6	42.0	11.1	12.7	17.3	25.8	19.3	24.9	59.3	116.5	48.0
1845.	105.3	93.3	29.0	31.2	26.9	20.2	16.2	18.5	21.9	30.5	62.3	69.2	43.7
1846.	99.4	67.3	53.4	35.5	33.9	19.0	24.3	19.3	21.0	32.4	33.3	81.6	43.8
1847.	176.9	92.6	54.8	43.6	19.1	20.8	36.3	22.0	22.9	30.5	40.3	48.2	51.3
1848.	80.9	40.5	88.7	44.3	46.5	21.3	31.2						
Mean.	118.3	77.1	51.0	39.4	27.5	18.6	25.1	20.3	22.4	27.5	59.3	71.6	46.8

TABLE LXVII.

Number of positive readings of the electric tension at sunset in each month from August 1843 to July 1848 inclusive.

Year.	Jan.	Feb.	Mar.	April.	May.	June.	July.	Aug.	Sept.	Oct.	Nov.	Dec.	Sums.
1843.	25	30	24	28	31	138
1844.	27	25	27	29	30	29	28	28	25	27	27	30	332
1845.	26	26	22	25	25	26	29	27	29	31	26	27	319
1846.	30	24	29	26	29	28	28	29	29	28	30	29	339
1847.	29	27	29	26	27	26	30	27	29	30	30	29	339
1848.	29	25	28	23	28	27	28		188
Sums.	141	127	135	129	139	136	143	136	142	140	141	146	1655

TABLE LXVIII.

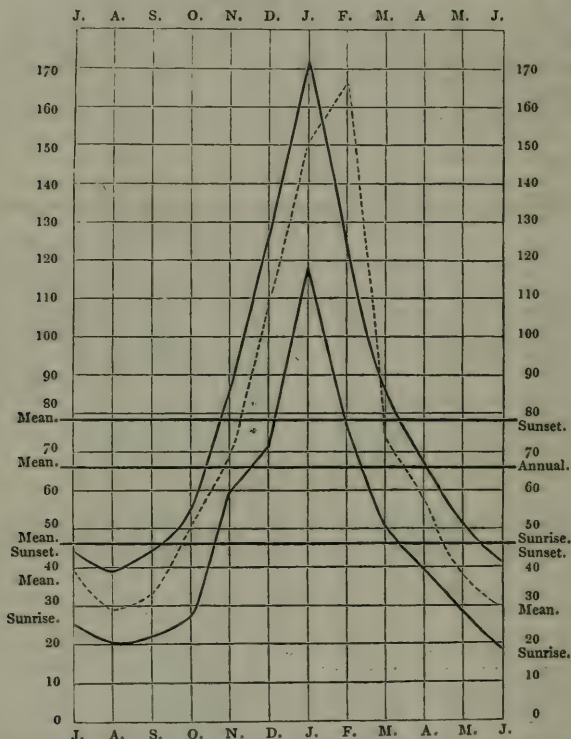
Mean electric tension at sunset in each month from August 1843 to July 1848 inclusive, with the mean monthly electric tension deduced from them.

Year.	Jan.	Feb.	Mar.	April.	May.	June.	July.	Aug.	Sept.	Oct.	Nov.	Dec.	Mean.
	div.	div.	div.	div.	div.	div.	div.	div.	div.	div.	div.	div.	div.
1843.	38.0	49.2	44.0	164.8	58.7	
1844.	173.1	156.5	48.7	39.3	32.1	34.6	38.3	40.1	46.3	56.8	58.6	225.0	79.0
1845.	90.2	151.2	60.6	61.3	43.5	34.2	29.8	43.4	50.7	57.2	74.4	84.4	64.6
1846.	86.5	78.1	162.1	75.1	41.5	44.0	46.8	35.6	34.4	72.9	60.1	181.2	76.7
1847.	342.2	170.5	66.5	50.7	58.4	37.0	54.9	39.4	42.7	42.2	78.3	86.3	89.6
1848.	158.6	61.3	81.7	124.1	82.0	61.2	50.0						
Mean.	171.2	124.8	85.6	68.2	51.3	42.2	44.0	39.3	44.6	54.8	87.1	127.4	78.4

The results of the five years' observations furnish the ordinates of two annual curves, viz. that exhibiting the annual period of the electrical tension at sunrise, and that exhibiting its annual period at sunset. As before remarked, the sunset curve is superior to that of sunrise. In fig. 17, these curves are projected on the same scale, so that the eye at once recognises the monthly differences between them. The annual curve, as deduced from the observations of 1845, 1846 and 1847, is also added for the sake of comparison. It will be observed that the three curves agree in presenting the slight increase of tension in July as compared with both June and August, which forms a secondary but very inferior maximum, and to which allusion has already been

made. The minimum tension at sunset occurs in August (value 39.3 div.), and is succeeded in September and October by a gentle and regular increase. From October to January the increase is very rapid, but at the same time very regular, so that the curve possesses a bold flowing character, the ascending branch being free from interruptions arising from *sudden* starts in

Fig. 17.



Curves representing the annual period of the electrical tension at sunrise, sunset, and the mean from the observations of 1845, 1846 and 1847.

the movement, or from sudden and irregular increments of tension. The apex which occurs in January is well-marked and acuminated in its character, and the portion of the descending branch immediately succeeding it is to a great extent symmetrical with the corresponding portion of the ascending branch, and this symmetry obtains at least between the months of November and March. The entire diminution of tension from January to June presents precisely the same characteristics as the increase from August to January, viz. regularity of decrement, giving to the curve a flowing character, which in consequence of the large differences in the monthly tensions also possesses considerable boldness.

The mean annual curve derived from the observations of 1845, 1846 and 1847, differs from the curve just examined in two or three minor particulars.

It is not so gracefully flowing in its character, although based on a greater number of observations, thereby indicating a certain irregularity of movement in the monthly increase and decrease of tension, doubtless dependent on the *accidental* electrical character of each individual month contributing to the mean, which accidental character, it is highly probable, is derived from certain disturbing influences to be noticed in the next section, the effects of which have been eliminated in the sunset curve by employing five instead of three years' observations. The apex of the mean annual curve of 1845 to 1847 occurs a month later, but from the high tension in January it would probably appear that from a longer series of observations, January and February would present an equality of tension, or January would become the superior. As it is, there is at this point a marked difference between the curves, the later occurrence of the apex in the three years' curve destroying to a great extent the symmetry so observable in the sunset curve. With the exceptions just noticed, the two curves in their general course are similar, and this would suggest that the sunset curve presents to a certain extent an approximation to the mean annual curve of electrical tension, but in excess. The monthly differences between the two are as follows.

TABLE LXIX.

Monthly differences between the annual periods at sunset (five years) and the mean of the years 1845, 1846 and 1847.

Jan.	Feb.	Mar.	April.	May.	June.	July.	Aug.	Sept.	Oct.	Nov.	Dec.	Mean.
div. +	div. -	div. +	div. +	div. +	div. +	div. +	div. +	div. +	div. +	div. +	div. +	div. +
20·5	41·8	10·6	11·0	13·4	12·9	5·2	9·9	11·6	4·3	17·5	17·9	11·5

It will be observed that these differences upon the whole are greater in winter than in summer, particularly in the months of November, December and January. During these months the epoch of sunset is nearer to 4 P.M. than at any other period of the year, and at this hour the electrical tension differs only 2·2 div. from the mean, being in excess. If we take the curve of the three years as representing the mean tension, then it would appear that the mean tension at sunset increases upon the mean of all the observations at the twelve daily readings during the winter as compared with the summer, to the extent of about 16 div. The following numbers express the ratio of the mean tension derived from the observations at the twelve observation-hours to the mean tension at sunset.

TABLE LXX.

Ratio of the mean electrical tension, as derived from the observations of 1845, 1846 and 1847, to the mean electrical tension, as derived from five years' observations at sunset for each month in the year.

Jan.	Feb.	Mar.	April.	May.	June.	July.	Aug.	Sept.	Oct.	Nov.	Dec.	Mean.
·880	1·334	·876	·838	·738	·694	·881	·748	·739	·921	·799	·859	·853

It will be observed that these ratios approach nearer to unity in the winter than in the summer, agreeing in this respect with the differences already noticed; but while the increasing differences denote divergence in the curves, the increasing ratios indicate a proportional convergence in the values of their ordinates. In winter the proportional value of the entire mean tension, 1849.

as compared with the sunset mean tension, is nearly two-tenths more than it is in summer, *i. e.* in summer the value of the entire mean tension is about seven-tenths of the sunset mean tension, while in winter it is nearer nine-tenths: there is a greater *proportional* difference in summer than in winter. That this ought to obtain is evident from the consideration that the epoch of mean tension as before-mentioned is 4 P.M. In the summer the epoch of sunset is nearer 8 P.M., at which hour the tension is higher than at 4 P.M., consequently the differences of the two sets of mean tension should from this variability of the epoch of sunset be greater in summer than in winter; and although this is not apparent when we contemplate the differences only, because of the great increase of tension in the winter, yet upon ascertaining the ratios of the one series to the other it becomes apparent, the proportional differences as we have seen being greater in summer than in winter.

The irregularity of the curve of entire mean tension renders it doubtful whether these ratios ought to be regarded as at all sufficiently approximate to justify their employment in deducing from a series of sunset observations the entire mean tension. The month of July presents its usual anomalous character. October also presents a higher tension than the usual flexure of the curve would indicate as the mean, and the displacement of the apex renders it difficult to apply any correction at present to the February mean; nevertheless it is highly probable that a scale of corrections founded on the distance of the epoch of sunset from that of the mean tension of the entire year, and applied to the deductions from five or more years' observations, would furnish a tolerable approximation to the annual period of the entire mean tension.

The lowest curve in fig. 17 is that derived from the observations at sunrise; it partakes greatly of the character of the sunset curve, the flowing of the ascending branch only being interrupted by a greater increment of electrical tension in November than the mean monthly increment at this period of the year, and this would appear to be confined to November 1843 (see Table LXVI. on page 159). With this exception the sunrise curve follows the sunset very closely, the principal difference being in range, the range of the sunset exceeding that of sunrise by 32.2 div.

We now come to examine the *differences* between the annual periods of sunrise and sunset. These periods differ from each other not only in value, but, as we have just observed, in range; the consequence is an inequality of the monthly differences between them. We have already alluded to the approximation of the sunset curve to the mean annual curve derived from the observations of three years at the twelve observation-hours, the necessary corrections being comparatively small; it is however probable that the curves of sunrise and sunset approach much nearer in *form* to the true annual curve, which in value would come between them, and it is also likely that both curves may furnish true representatives of the annual period when certain corrections are applied, the value and range of the sunrise being necessarily lower than those of the sunset, from the observations contributing to its determination being made at a portion of the day characterized by a *feebler* development of the electrical tension. As the tension increases towards sunset in a certain ratio and according to a certain law which is most probably preserved during the annual progression of the electrical tension, the consequence would be that with increasing tensions at sunset we should have increasing differences from summer to winter and decreasing differences from winter to summer, and that from a sufficiently long series of observations either at sunset, sunrise, or any selected hour, the mean annual period might be deduced. The following are the differences between the two series.

TABLE LXXI.

Monthly differences between the annual periods at sunrise and sunset.

Jan.	Feb.	Mar.	April.	May.	June.	July.	Aug.	Sept.	Oct.	Nov.	Dec.	Mean.
52·9	47·7	34·6	28·8	23·8	23·6	18·9	19·0	22·2	27·3	27·8	55·8	31·6

It will be remarked that these differences gradually decrease from December to July and gradually increase from July to December, but it should not be forgotten that the variability of the epochs of sunrise and sunset has a great tendency to produce a difference between the mean monthly values of the tensions of the two periods in the contrary direction, *i. e.* a greater difference in June and a less difference in December. In the summer, when we have the least difference in consequence of the general low tension of the season, the epochs of observation are the furthest removed from each other, that of sunrise nearly coinciding with or being but little removed from the epoch of the principal minimum, and that of sunset being brought within two hours of the epoch of the principal maximum: under these circumstances, and leaving out of consideration the effects of other movements, we ought to have the greatest difference between the tensions. On the other hand, in winter the epoch of sunrise occurs within two hours of the forenoon maximum, and that of sunset nearly coincides with the afternoon minimum; it is consequently manifest that the differences existing under these circumstances should be the least, and the entire series of monthly differences ought to enter as corrections in deducing the true annual period from observations at sunrise and sunset. It is however clear, from the series of differences before us, that this object cannot be attained by a mode of discussion which regards *them* only, the two opposite series of differences being mingled together in those presented to our notice; but if we compute the ratios of the sunrise to the sunset mean tensions, we shall probably discover the effects of the recess and approach of the epochs of observation according to the season of the year, the further they are removed from each other the lower the ratio—coincidence of value being considered as unity—and on the contrary the nearer they approach each other the higher the ratio, the proportional difference being less. The following numbers clearly exhibit these proportional differences.

TABLE LXXII.

Ratio of the mean electrical tension at sunrise to that at sunset, for each month of the year.

Jan.	Feb.	Mar.	April.	May.	June.	July.	Aug.	Sept.	Oct.	Nov.	Dec.	Mean.
·691	·618	·596	·578	·536	·440	·570	·517	·502	·501	·680	·562	·596

In these ratios the effect of the variability of the epochs of observation is very apparent, the difference between June and January being as much as ·251, which is more than one-third of the ratio in January. In June the mean tension at sunrise is less than one-half of the mean tension at sunset; in January it is considerably more than in June, being very nearly seven-tenths of the sunset mean tension. There are two or three anomalies in the

numbers, apparently arising from the sunrise observations, which must render a more extended series necessary before a correct scale of ratios between the two series can be computed.

SECTION 3.—*Discussion of Observations between August 1, 1843, and December 31, 1844.*

The entire number of observations selected from the seventeen months constituting the earlier portion of the five years is 1897; of these 551 were made in the last five months of 1843, and 1346 in the year 1844. It is quite improbable that from a series of this kind an annual period could be deduced. The only epochs of observation adopted in 1843 and 1844, and continued during the remainder of the five years, were sunrise and sunset; they accordingly, of all the observations, furnish an unbroken series during the whole period, and as such have been already examined. The epochs of the remaining observations, 9 A.M. and 3 P.M., having been discontinued at the end of 1844, the results furnished by them are necessarily very partial. In the discussion of 1843 and 1844 they have been incorporated with those of sunrise and sunset, and have been employed in the first instance in deducing the mean tensions in Table LXXIII., which are those of each day on which positive electricity *only* as a general rule has been observed.

TABLE LXXIII.

Mean electrical tension of each day from August 1, 1843, to December 31, 1844, on which positive electricity *only* was observed.

D.	Aug.	Sept.	Oct.	Nov.	Dec.	Jan.	Feb.	Mar.	April.	May.	June.	July.	Aug.	Sept.	Oct.	Nov.	Dec.
	div.	div.	div.	div.	div.	div.	div.	div.	div.	div.	div.	div.	div.	div.	div.	div.	div.
1	19.75	39.25	14.25	85.25	33.75	..	111.25	41.25	425.	38.75	11.	146.25	20.	44.25	43.12
2	..	29.	18.50	425.	77.50	154.75	375.	42.50	162.50	31.25	30.75	98.75	14.37	..	65.62
3	..	21.75	18.25	59.50	39.25	925.	372.50	27.50	66.25	27.12	31.	32.	..	30.37	6.25	..	30.62
4	..	14.50	12.75	158.75	8.50	600.	407.50	..	30.75	22.75	21.	..	16.37	19.62	34.12	49.37	65.62
5	18.75	34.25	33.25	31.	17.50	15.	450.	76.25	..	56.50	23.	..	48.12	18.37	3.87	160.62	450.
6	18.	62.25	..	34.75	42.25	20.75	425.	77.50	54.	64.75	5.25	21.87	20.37	25.	16.37	71.25	800.
7	27.75	58.	27.	22.	17.25	100.	..	38.75	68.12	27.50	8.	28.25	17.37	30.25	32.50	63.	216.25
8	22.75	62.50	18.	62.75	50.	51.25	55.	51.66	73.33	11.	25.50	37.50	12.50	52.87	57.50	..	71.25
9	35.50	39.25	..	158.75	63.75	40.	..	12.75	56.25	21.	13.25	25.	15.75	29.50	3.75	46.87	35.25
10	5.50	..	51.25	33.50	22.25	33.25	48.75	..	32.75	17.50	15.12	34.37	52.50	45.50	21.37	83.75	50.62
11	46.25	13.	..	345.	25.	52.50	66.25	..	43.50	30.75	23.	24.87	29.87	43.25	19.75	58.12	66.87
12	15.	46.25	..	447.50	168.75	..	68.75	..	33.87	23.12	23.87	30.50	29.87	32.75	39.12	16.5	76.87
13	48.75	62.25	27.50	428.75	225.	..	743.75	42.50	..	28.25	10.50	17.25	30.87	34.12	27.50	..	23.62
14	24.75	77.50	41.	233.75	175.	..	205.	40.62	15.75	21.50	10.25	26.	..	33.50	16.25	76.25	55.62
15	..	33.	51.25	245.	28.25	65.	9.75	28.	23.25	13.50	14.	20.62	20.87	4.50	..	12.50	195.
16	..	58.75	27.	43.75	17.50	375.	61.25	15.75	24.75	21.50	21.25	39.62	41.87	3.87	41.66	22.50	178.25
17	51.25	63.75	30.	116.25	112.50	300.	15.50	6.50	25.75	16.75	10.75	35.62	41.87	60.62	38.12	37.50	42.50
18	66.33	47.50	21.25	68.25	32.50	38.75	8.50	35.	33.50	7.50	10.75	35.62	41.87	60.62	38.12	37.50	42.50
19	36.25	39.25	107.50	198.75	77.50	42.50	26.75	24.	33.50	7.50	10.75	35.62	41.87	60.62	38.12	37.50	42.50
20	5.75	61.25	43.50	..	17.25	38.	36.25	36.66	12.	15.87	13.25	16.37	14.25	..	16.75	37.50	62.50
21	27.25	55.	..	10.75	13.	60.	..	72.50	28.25	15.	40.37	32.37	30.	15.33	17.83	192.50	57.50
22	..	37.75	38.75	53.33	7.75	400.	58.75	33.75	21.	14.75	25.75	41.25	20.62	..	39.	212.50	160.
23	25.	48.75	38.50	..	20.25	31.	38.75	15.	49.37	63.12	35.	162.50	68.75
24	7.75	24.25	20.75	300.	20.	203.75	37.50	17.25	12.50	16.37	21.25	51.62	35.87	..	55.83	97.50	58.75
25	28.	19.25	43.25	..	42.50	46.25	53.75	1.50	26.75	16.12	..	39.12	30.62	33.50	45.66	111.25	56.25
26	20.50	24.75	104.75	18.25	26.25	20.25	30.25	21.75	159.50	15.25	30.	12.75	18.87	60.	23.75	136.25	375.
27	48.75	15.25	65.	26.	49.50	42.	52.50	13.	22.	15.75	28.75	16.25	31.25	50.	59.37	180.62	650.
28	..	19.75	..	23.25	146.25	13.25	59.75	34.50	33.12	8.75	27.62	26.62	52.50	41.87	243.75	109.37	183.75
29	3.25	38.75	116.25	33.75	55.	28.75	83.75	76.25	38.75	11.	31.25	9.50	60.	6.75	71.25	41.50	18.33
30	36.	8.50	..	112.50	201.25	32.75	..	53.12	45.50	..	38.	21.16	55.62	39.37	66.25	49.37	408.12
31	28.75	38.75	51.50	..	19.75	..	19.87	27.50

From this table has been formed Table LXXIV., which contains the greatest and least mean electrical tensions observed in each of the seventeen months, with their differences, and the days on which they occurred.

TABLE LXXIV.

Greatest and least mean daily electrical tension in each month, from August 1843 to December 1844, both inclusive, with their differences, and days of the month on which they occurred.

Month.	Mean daily electrical tension.		Difference.	Days of the month on which the mean electrical tension was	
	Greatest.	Least.		Greatest.	Least.
1843 and 1844.					
	div.	div.	div.		
August	66·33	3·25	63·08	18	29
September	77·50	8·50	69·00	14	30
October	116·25	12·75	103·50	29	4
November	447·50	10·75	436·75	12	21
December	225·00	7·75	217·25	13	22
January	925·00	13·25	911·75	3	28
February	743·75	8·50	735·25	13	18
March	77·50	1·50	76·00	6	25
April	425·00	12·00	413·00	1	20
May	64·75	7·50	57·25	6	19
June	49·37	5·25	44·12	23	6
July	63·12	9·50	53·62	23	29
August	60·00	12·50	47·50	29	8
September	146·25	3·87	142·38	1	16
October	243·75	3·75	240·00	28	9
November	212·50	12·50	200·00	22	15
December	800·00	18·33	781·66	6	29

The greatest mean daily electrical tension occurred in January 1844, and the least in March 1844: the difference (923·5 div.) is the *range* of the mean tensions during the seventeen months.

The numbers in this table bear testimony to the same general fact which we have already noticed in the discussion of the three years' observations, viz. the great increase of electric tension in winter; but from the nature of the quantities recorded, they are not comparable with the annual curves deduced from the observations of 1845, 1846 and 1847, and from those of sunrise and sunset during the five years.

From Table LXXV. we learn that in every month the electrical tension exceeded 79 div. of Volta's electrometer No. 1. In November 1843, January, February, March, April, and December 1844, the highest observed tensions at the four observation-epochs were between 1000 div. and 1500 div., or between 10° and 15° of Henley's instrument. In the remaining months, with the exception of August 1843 and June 1844, the highest tensions were between 100 div. and 500 div., or 1° and 5° of Henley, and in the two excepted months they were respectively 95 div. and 80 div. The effect of the annual progression is very apparent, the higher tensions being confined to the winter months.

During the seventeen months the electrical tension was never observed below 2 div. of Volta No. 1, except on one or two occasions on which the tension was too feeble materially to influence the instrument. The numbers in the column of least absolute tensions give the lowest observed tensions by Volta's instrument in the respective months.

Tables LXXVI. and LXXVII. exhibit the monthly distribution of all the observations at the four observation-epochs, together with the value of the mean electrical tension at each observation-epoch in each of the seventeen months.

TABLE LXXV.

Greatest and least electrical tension observed in each month, from August 1843 to December 1844, both inclusive, with their differences, and the days of the month on which they occurred.

Month.	Absolute electrical tension in each month.		Difference.	Days of the month on which the electrical tension was	
	Greatest.	Least.		Greatest.	Least.
1843 and 1844.					
	div.	div.	div.		
August	95	2	93	{ 17 9 a.m. 18 3 p.m. 7 9 a.m.	29 3 p.m.
September	115	3	112	{ 16 9 a.m. 20 9 a.m. 21 9 a.m.	30 S.R.
October	300	3	297	{ 19 9 a.m. 26 9 a.m.	{ 1 S.R. 3 S.S. 4 S.R. 16 S.R. 23 S.R. 29 S.R.
November	1100	5	1095	12 S.S.	{ 4 3 p.m. S.S. 22 3 p.m.
December	500	2	498	14 9 a.m.	10 S.R.
January	1200	3	1197	4 9 a.m.	17 S.R.
February	1000	3	997	4 9 a.m.	{ 18 9 a.m. 9 S.R. 9 a.m. 17 S.R. 25 S.R. 9 a.m.
March	1500	3	1497	8 9 a.m.	16 S.R.
April	1000	3	997	1 9 a.m.	17 S.R.
May	200	2	198	{ 5 S.S. 6 9 a.m.	22 S.S.
June	80	2	78	30 9 a.m.	7 S.R.
July	100	5	95	23 S.S.	{ 4 S.R. 6 S.S. 7 S.S. 26 S.R. 31 S.R.
August	105	4	101	29 9 a.m.	6 S.R.
September	400	3	397	1 9 a.m.	16 9 a.m. S.S.
October	400	2	398	28 S.S.	9 3 p.m.
November	500	4	496	27 9 a.m.	16 3 p.m.
December	1100	3	1097	6 3 p.m.	16 9 a.m.

TABLE LXXVI.

Number of positive readings at each observation-epoch in each month, from August 1843 to December 1844, both inclusive.

Epochs.	Aug.	Sept.	Oct.	Nov.	Dec.	Jan.	Feb.	Mar.	Apr.	May.	June.	July.	Aug.	Sept.	Oct.	Nov.	Dec.	Sums.
Sunrise ..	26	30	24	27	31	28	28	27	30	31	30	30	28	24	28	28	31	481
9 a.m. ...	26	30	24	28	31	26	28	27	29	31	30	30	28	25	29	28	30	480
3 p.m. ...	25	29	23	28	31	28	28	26	29	30	27	26	27	25	27	27	30	466
Sunset ..	25	30	24	28	31	27	25	27	29	30	29	28	28	25	27	27	30	470
Sums	102	119	95	111	124	109	109	107	117	122	116	114	111	99	111	110	121	1897

TABLE LXXVII.

Mean electrical tension at each observation-epoch in each month, from August 1843 to December 1844, both inclusive, with the mean annual period of 1844.

Epochs.	Aug.	Sept.	Oct.	Nov.	Dec.	Jan.	Feb.	Mar.	April.	May.	June.	July.	Aug.	Sept.	Oct.	Nov.	Dec.	Mean.
Sunrise ..	div. 15·7	div. 26·1	div. 17·4	div. 105·0	div. 40·7	div. 127·0	div. 93·9	div. 23·6	div. 42·0	div. 11·1	div. 12·7	div. 17·3	div. 25·8	div. 19·3	div. 24·9	div. 59·3	div. 116·5	div. 46·1
9 a.m. ...	34·2	57·0	73·9	188·6	77·3	173·1	189·3	96·0	128·4	30·8	24·9	38·5	38·1	57·4	43·3	138·7	175·8	91·5
3 p.m. ...	18·0	26·9	29·5	86·6	64·8	142·6	151·5	35·8	33·8	12·0	18·1	20·8	23·8	35·1	38·4	66·1	204·8	60·9
Sunset ..	38·0	49·2	44·0	164·8	58·7	173·1	156·5	48·7	39·3	32·1	34·6	38·3	40·1	46·3	56·8	58·6	225·0	76·9
Mean	26·4	39·9	41·3	136·5	60·4	153·4	147·5	51·2	60·7	21·5	22·6	28·8	32·0	39·7	40·7	81·0	180·0	68·9

The numbers expressing the mean electrical tension of each month exhibit very clearly a mean annual period, which may be advantageously compared with the annual periods already deduced; for this purpose the four annual periods derived from various sources are included in the following table.

TABLE LXXVIII.

Comparison of the annual periods of the electric tension derived from various sources.

Annual period.	Jan.	Feb.	Mar.	Apr.	May.	June.	July.	Aug.	Sep.	Oct.	Nov.	Dec.	Mean.
	div.	div.	div.	div.	div.	div.	div.	div.	div.	div.	div.	div.	div.
Sunset, 5 years	171·2	124·8	85·6	68·2	51·3	42·2	44·0	39·3	44·6	54·8	87·1	127·4	78·4
3 years, 1845 to 1847.	150·7	166·6	75·0	57·2	37·9	29·3	38·8	29·4	33·0	50·5	69·6	109·5	66·9
Sunrise, 5 years	118·3	77·1	51·0	39·4	27·5	18·6	25·1	20·3	22·4	27·5	59·3	71·6	46·8
The year 1844.....	153·4	147·5	51·2	60·7	21·5	22·6	28·8	32·0	39·7	40·7	81·0	180·0	71·8

Upon comparing the annual period deduced from the four daily readings, in the year 1844, with those recorded in Table LIV., we find the same irregularity of movement which characterized each of those deduced from the twelve daily readings in 1845, 1846 and 1847. The contrast in this respect with the smoothness and regularity in the general flowing of the curves, derived from five years' observations, appears to indicate that this is the shortest term in which the effects of accidental influences may be efficiently eliminated, so as to exhibit the annual progression of the electric tension, either in immediate connexion with, or following at some definite interval, the annual progression of the humidity of the atmosphere. We have already alluded to the protuberance on the upward branch of the sunrise curve, as resulting from a higher tension than ordinary in the month of November 1843 (see p. 162). The fifth column (Nov.) in Table LXXVII. exhibits the extraordinary character of this month, and shows that the electric tension was developed with increased force at each of the observation-epochs: this is very apparent from the comparison of this month with the remaining Novembers, and from it we may infer, that upon five years' observations, the tension of November being of the ordinary character, the annual curve is likely to present a smooth and gently flowing contour. In the table before us the features of the summit of the annual curve are well-marked: we have already alluded to the acuminate and symmetrical character of the summit of the sunset curve (see p. 160). This is amply borne out by the annual curve

of the year 1844, and indeed by the others. Compared with the entire year, the three months, December, January and February, present by far the greatest electrical tension. In shorter intervals than five years, the months of maximum vary, sometimes occurring in the one or the other of the three months; but it appears from the entire series of five years, that the greatest tension is confined to the three months above-named.

The shortness of the period over which the observations at 9 A.M. and 3 P.M. extend, combined with the *irregularity* appertaining to the movements of a single year, render it impracticable to deduce the relation existing between the values at those fixed epochs. Nothing further than the general fact, confirmatory of the results deduced from the observations of 1845 to 1847, viz. that the tension in the forenoon hours is higher than that in the afternoon, is likely to be attained. This general result, which is very striking, is exhibited in Table LXXIX.

During the entire period the electric tension increased from sunrise to 9 A.M.; the mean value of this increase on the seventeen months is 45·4 div.: this, however, cannot be considered as of equal importance with the mean of the year, because the last five months of the year 1843 contribute to its determination. With only one exception, viz. December 1844, the tension declined from 9 A.M. to 3 P.M.—mean value as before, 30·6 div. It is not to be considered that the tension *actually* declines from 9 to 3, for we have already seen that 10 A.M. is the usual epoch of the forenoon maximum, but that the tension on an average is lower at 3 P.M. than at 9 A.M. The table shows an increase from 3 P.M. to sunset, with two exceptions: December 1843 and November 1844, mean value as before, 16·0 div., with the same limitation as to the character of the increase, 4 P.M. being the usual epoch of the afternoon minimum. These movements are further illustrated by the next table, which exhibits the excess or defect of the mean electrical tension above or below the mean of each month.

There are two or three numbers in the above columns that require a passing notice; most of them proceed very regularly, exhibiting a higher tension than the mean at 9 A.M. and sunset, and a lower tension at sunrise and 3 P.M. The first exception that we have to this order is in December 1843, the mean tension at 3 P.M. being in *excess*, while that at sunset is in *defect*. In this month the double progression disappears, the tension declining 18·6 div. from 9 A.M. to sunset. The second exception occurs in February 1844, when the tension at 3 P.M. was 4·0 div. *higher* than the mean; the usual order of progression was not interrupted; but from Table LXXVII. it would appear that the increase of tension giving rise to the anomaly just noticed, occurred principally between sunrise and 9 A.M., and was maintained afterwards. March and April 1844 present similar exceptions to each other in the tension at sunset being below the mean; the usual course of progression was not, however, interrupted in either case, as appears from Table LXXVII. The next exception occurs in November 1844, the tension at sunset being 22·4 div. below the mean: an inspection of Table LXXVII. indicates that the increase of tension, as in the former instances, took place between sunrise and 9 A.M., *but was not maintained afterwards*—in fact a diminution instead of an increase occurred at sunset; the increase between sunrise and 9 A.M. augmented the value of the monthly mean tension, and this, combined with the reversal of the usual movement at sunset, occasioned the depression of the mean at sunset below the mean of the month. In December 1844 there are no traces of the double progression, the tension increasing from sunrise to sunset: the epoch of mean tension for the month occurs between 9 A.M. and 3 P.M.; the signs of these mean quan-

TABLE LXXIX.—Differences between the mean electrical tensions at the four daily epochs from August 1843 to December 1844, both inclusive.

Epochs.	Aug.	Sept.	Oct.	Nov.	Dec.	Jan.	Feb.	March.	April.	May.	June.	July.	Aug.	Sept.	Oct.	Nov.	Dec.	Mean.
Sunrise to 9 a.m.	div. + 18.5	div. + 30.9	div. + 56.5	div. + 83.6	div. + 36.6	div. + 46.1	div. + 95.4	div. + 72.4	div. + 86.4	div. + 19.7	div. + 12.2	div. + 21.2	div. + 12.3	div. + 38.1	div. + 18.4	div. + 79.4	div. + 59.3	div. + 45.4
9 a.m. to 3 p.m....	— 16.2	— 30.1	— 44.4	— 102.0	— 12.5	— 30.5	— 37.8	— 60.2	— 94.6	— 18.8	— 6.8	— 17.7	— 14.3	— 22.3	— 4.9	— 72.6	— 29.0	— 30.6
3 p.m. to sunset...	+ 20.0	+ 22.3	+ 14.5	+ 78.2	— 6.1	+ 30.5	+ 5.0	+ 12.9	+ 5.5	+ 20.1	+ 16.5	+ 17.5	+ 16.3	+ 11.2	+ 18.4	— 7.5	+ 20.2	+ 16.0

TABLE LXXX.—Excess or defect of the mean electrical tension at each observation-epoch, as compared with the mean of each month, from August 1843 to December 1844, both inclusive, and also with the mean of the seventeen months.

Epochs.	Aug.	Sept.	Oct.	Nov.	Dec.	Jan.	Feb.	March.	April.	May.	June.	July.	Aug.	Sept.	Oct.	Nov.	Dec.	Mean.
Sunrise	div. — 10.7	div. — 13.8	div. — 23.9	div. — 31.5	div. — 19.7	div. — 26.4	div. — 53.6	div. — 27.6	div. — 18.7	div. — 10.4	div. — 9.9	div. — 11.5	div. — 6.2	div. — 20.4	div. — 15.8	div. — 21.7	div. — 63.5	div. — 22.8
9 a.m.	+ 7.8	+ 17.1	+ 32.6	+ 52.1	+ 16.9	+ 19.7	+ 41.8	+ 44.8	+ 67.7	+ 9.3	+ 2.3	+ 9.7	+ 6.1	+ 17.7	+ 2.6	+ 57.7	+ 4.2	+ 22.6
3 p.m.	— 8.4	— 13.0	— 11.8	— 49.9	+ 4.4	— 10.8	+ 4.0	— 15.4	— 26.9	— 9.5	— 4.5	— 8.0	— 8.2	— 4.6	— 2.3	— 14.9	+ 24.8	— 8.0
Sunset	+ 11.6	+ 9.3	+ 2.7	+ 28.3	— 1.7	+ 19.7	+ 9.0	— 2.5	— 21.4	+ 10.6	+ 12.0	+ 9.5	+ 8.1	+ 6.6	+ 16.1	+ 22.4	+ 45.0	+ 8.0

tities are consequently reversed. It is worthy of remark that, while both the Decembers present *single* progressions, the points of maxima occur at different periods of the day. The very close approximation of the values of the means of the seventeen months, with opposite signs at sunrise and 9 A.M. and at 3 P.M. and sunset, is very interesting, and strongly indicates a symmetrical disposition of the epochs of mean tension, as deduced from these four daily readings with regard to them. We have, in fact, a tension at 9 A.M. as much above the mean as that at sunrise is below it, and the same thing occurs at 3 P.M. and sunset, the value being rather more than one-third of the movements in the morning. The great accordance which exists in this respect with the results already arrived at is interesting, and tends greatly to confirm the deductions that the movements are much greater in the morning and forenoon, that the depression in the afternoon is of a minor character, and that the principal maximum occurs in the evening; for although the mean tension at sunset is *lower* than that at 9 A.M. (see Table LXXXVII.), yet the occurrence of two winters in the seventeen months tends to place the epoch of sunset *nearer* the afternoon minimum than it is upon the year. It should be borne in mind, that as the epochs of sunrise and sunset are variable, those of mean tension must be symmetrically disposed with regard to the *mean epochs of sunrise and sunset*, and 9 A.M. and 3 P.M.

High tensions.—Of the 1346 observations in the year 1844 that contribute to the results deduced for that year, 180 belong to the class of high tensions. The two following tables exhibit the distribution and limits of value of these readings.

TABLE LXXXI.

Number of positive readings (high, or above 60 div.) at each observation-epoch in each month of the year 1844.

Month.	Sunrise.	9 a.m.	3 p.m.	Sunset.	Sums.
January	8	6	8	7	29
February ..	11	9	5	6	31
March	4	3	...	1	8
April	5	5	1	...	11
May	1	...	1	2
June					
July					
August					
September ..	2	2	4
October	2	1	1	1	5
November ..	11	13	7	6	37
December ..	12	13	14	14	53
Sums	55	53	36	36	180

TABLE LXXXII.—Limits of value of the *high* readings at each observation-epoch in the year 1844.

At Sunrise.		
Jan.		
	div.	div.
1 at	95.	
5 between 100 and		500 inclusive.
1 at		800.
1 at		1000.
Feb.		
3 at	75.	
3 between 75 and		100.
5 between 100 and		500 inclusive.
March.		
4 between 75 and		90 inclusive.
April.		
4 between 60 and		110 inclusive.
1 at		500.
Sept.		
2 between 70 and		75 inclusive.
Oct.		
2 at	75.	
Nov.		
9 between 65 and		100 inclusive.
2 between 200 and		300 inclusive.
Dec.		
4 between 75 and		85 inclusive.
7 between 200 and		500 inclusive.
1 at		600.

55

At 9 A.M.		
Jan.		
	div.	div.
4 between 100 and		500 inclusive.
1 at		1000.
1 at		1200.
Feb.		
6 between 120 and		500 inclusive.
2 at		700.
1 at		1000.
March.		
1 at	90.	
1 at	110.	
1 at		1500.
April.		
4 between 115 and		500 inclusive.
1 at		1000.
May.		
1 at	200.	
Sept.		
1 at	200.	
1 at		400.
Oct.		
1 at	200.	
Nov.		
2 at	85.	
11 between 100 and		500 inclusive.
Dec.		
5 between 55 and		95 inclusive.
6 between 200 and		500 inclusive.
1 at		700.
1 at		900.

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At 3 P.M.		
Jan.		
	div.	div.
7 between 100 and		500 inclusive.
1 at		900.
Feb.		
3 between 200 and		500 inclusive.
1 at		600.
1 at		1300.
April.		
1 at	300.	
Oct.		
1 at	300.	
Nov.		
1 at	75.	
1 at	90.	
5 between 100 and		300 inclusive.
Dec.		
3 between 70 and		100 inclusive.
8 between 200 and		500 inclusive.
1 at		600.
1 at		900.
1 at		1100.

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At Sunset.		
Jan.		
	div.	div.
5 between 100 and		500 inclusive.
1 at		1000.
1 at		1100.
Feb.		
4 between 300 and		500 inclusive.
1 at		600.
1 at		900.
March.		
1 at	200.	
May.		
1 at	200.	
Oct.		
1 at	400.	
Nov.		
4 between 50 and		95 inclusive.
2 at		200.
Dec.		
3 between 40 and		90 inclusive.
5 between 100 and		500 inclusive.
4 at		600.
1 at		800.
1 at		900.

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Upon consulting Table LXXXII. it will be seen that in most instances the months March to October inclusive present tensions considerably lower in value than those of the remaining four months, and in connexion with this, Table LXXXI. informs us that in June, July and August, all the tensions were low. The means have accordingly been taken for January, February, the eight months of low tension, November and December; they are exhibited in the following table.

TABLE LXXXIII.

Mean electrical tension (high, or above 60 div.) at each observation-epoch for the months specified in the year 1844.

Epoch.	January.	February.	Eight months.	November.	December.	Mean.
	div.	div.	div.	div.	div.	div.
Sunrise	386·9	198·6	108·8	110·7	251·7	198·8
9 a.m.	616·7	480·0	426·2	243·8	337·3	390·4
3 p.m.	389·4	620·0	300·0	137·9	375·0	361·9
Sunset	528·6	500·0	266·7	113·3	413·9	388·2
Mean	469·3	406·6	264·3	163·0	348·1	325·7

TABLE LXXXIV.

Differences between the mean electrical tension (high, or above 60 div.) at the four daily epochs for the months specified in the year 1844.

Epoch.	January.	February.	Eight months.	November.	December.	Mean.
	div.	div.	div.	div.	div.	div.
Sunrise to 9 a.m.	+229·8	+281·4	+317·4	+133·1	+85·6	+191·6
9 a.m. to 3 p.m.	-227·3	+140·0	-126·2	-105·9	+37·7	- 28·5
3 p.m. to sunset.	+139·2	-120·0	- 33·3	- 24·6	+38·9	+ 26·3

TABLE LXXXV.

Excess or defect of the mean electrical tension (high, or above 60 div.) at each observation-epoch, as compared with the means of the winter months and also with the means of the eight months of low tension and of the entire year.

Epoch.	January.	February.	Eight months.	November.	December.	Mean.
	div.	div.	div.	div.	div.	div.
Sunrise	- 82·4	-208·0	-155·5	-52·3	-96·4	-126·9
9 a.m.	+147·4	+ 73·4	+161·9	+80·8	-10·8	+ 64·7
3 p.m.	- 79·9	+213·4	+ 35·7	-25·1	+26·9	+ 36·2
Sunset	+ 59·3	+ 93·4	+ 2·4	-49·7	+65·8	+ 62·5

These tables confirm the general result of the discussion of the high tensions in 1845, 1846 and 1847, viz. the *irregularity* of the movements above 60 div. In the four months specified the quantities are affected by precisely the same signs as those in Tables LXXIX. and LXXX., with only one exception, and thus we have additional evidence that the higher tensions materially influence the aggregate results.

Low tensions.—Upon omitting the high readings above specified, we obtain a series of numbers considerably in accordance with those recorded in Tables XXXIV. XXXV. XXXVI. and XXXVII.

The following table exhibits the distribution of the low readings in the year 1844, upon which the mean quantities in Table LXXXVII. are based.

TABLE LXXXVI.—Number of positive readings (low tension) at each observation-epoch in each month of the year 1844.

Epoch.	Jan.	Feb.	Mar.	April.	May.	June.	July.	Aug.	Sept.	Oct.	Nov.	Dec.	Sums.
Sunrise..	20	17	23	25	31	30	30	28	22	26	17	19	288
9 a.m....	20	19	24	24	30	30	30	28	23	28	15	17	288
3 p.m....	20	23	26	28	30	27	26	27	25	26	20	16	294
Sunset ..	20	19	26	29	29	29	28	28	25	26	21	16	296
Sums ...	80	78	99	106	120	116	114	111	95	106	73	68	1166

The numbers in this table are perfectly in accordance with those entering into the discussion of the low tensions of 1845, 1846 and 1847, in exhibiting the greatest quantity in the summer months.

TABLE LXXXVII.

Mean electrical tension (low, or below 60 div.) at each observation-epoch in each month of the year 1844, with the monthly and yearly mean tensions.

Epoch.	Jan.	Feb.	Mar.	April.	May.	June.	July.	Aug.	Sept.	Oct.	Nov.	Dec.	Mean.
Sunrise .	div. 23·0	div. 26·1	div. 13·8	div. 18·4	div. 11·1	div. 12·7	div. 17·3	div. 25·8	div. 14·5	div. 21·0	div. 26·1	div. 31·1	div. 19·2
9 a.m....	40·1	51·6	37·2	54·5	23·2	24·9	38·5	38·1	37·7	37·7	47·5	52·4	38·9
3 p.m....	43·9	49·6	35·8	24·3	12·0	18·1	20·8	23·8	35·1	28·3	40·9	55·9	30·6
Sunset ..	48·7	48·0	42·9	39·3	26·3	34·6	38·3	40·1	46·3	43·6	42·9	59·7	41·4
Means...	38·9	44·6	32·9	33·9	18·5	22·6	28·8	32·0	33·5	32·7	39·4	49·0	32·6

TABLE LXXXVIII.—Differences between the mean electrical tension (low, or below 60 div.) at the four daily epochs in each month of the year 1844.

Epoch.	Jan.	Feb.	Mar.	April.	May.	June.	July.	Aug.	Sept.	Oct.	Nov.	Dec.	Mean.
S.R. to 9 a.m....	div. + 17·1	div. + 25·5	div. + 23·4	div. + 36·1	div. + 12·1	div. + 12·2	div. + 21·2	div. + 12·3	div. + 23·2	div. + 16·7	div. + 21·4	div. + 21·3	div. + 19·7
9 a.m. to 3 p.m..	+ 3·8	+ 2·0	+ 1·4	+ 30·2	+ 11·2	+ 6·8	+ 17·7	+ 14·3	+ 2·6	+ 9·4	+ 6·6	+ 3·5	+ 8·3
3 p.m. to S.S....	+ 4·8	+ 1·6	+ 7·1	+ 15·0	+ 14·3	+ 16·5	+ 17·5	+ 16·3	+ 11·2	+ 15·3	+ 2·0	+ 3·8	+ 10·8

TABLE LXXXIX.—Excess or defect of the mean electrical tension (low, or below 60 div.) at each observation-epoch, as compared with the mean of each month and also with the mean of the year.

Epoch.	Jan.	Feb.	Mar.	April.	May.	June.	July.	Aug.	Sept.	Oct.	Nov.	Dec.	Mean.
Sunrise .	div. — 15·9	div. — 18·5	div. — 19·1	div. — 15·5	div. — 7·4	div. — 9·9	div. — 11·5	div. — 6·2	div. — 19·0	div. — 11·7	div. — 13·3	div. — 17·9	div. — 13·4
9 a.m....	+ 1·2	+ 7·0	+ 4·3	+ 20·6	+ 4·7	+ 2·3	+ 9·7	+ 6·1	+ 4·2	+ 5·0	+ 8·1	+ 3·4	+ 6·3
3 p.m....	+ 5·0	+ 5·0	+ 2·9	+ 9·6	+ 6·5	+ 4·5	+ 8·0	+ 8·2	+ 1·6	+ 4·4	+ 1·5	+ 6·9	+ 2·0
Sunset ..	+ 9·8	+ 3·4	+ 10·0	+ 5·4	+ 7·8	+ 12·0	+ 9·5	+ 8·1	+ 12·8	+ 10·9	+ 3·5	+ 10·7	+ 8·8

The double progression as a general rule is very apparent in the above tables. To the increase of tension from sunrise until 9 A.M. there are no exceptions; two only to the diminution of tension between 9 A.M. and 3 P.M., and one only to the increase from 3 P.M. to sunset. The numbers are greatly in harmony at least in this respect with the results obtained from the discussion of the low tensions in the years 1845, 1846 and 1847.

The exceptions to the double progression occurring as they do in the *winter* months, viz. January max. at sunset, February max. at 9 A.M., and December max. at sunset, are not without significance. In the discussion of the low tensions in the years 1845, 1846 and 1847, we adverted to the tendency exhibited by the winter curves to present a *single* progression; and while we noticed that the essential features of the *double* progression as exhibited in the aggregate curves were *compressed* as it were into the low tensions in the *summer* months, we also remarked that when the higher tensions—which were evidently more intimately connected with aqueous vapour, and were very much more numerous in the winter,—were withdrawn, the features of the double progression were withdrawn also, and suggested that upon a mode of directly observing the electrical tension of the aqueous vapour being devised, it might probably be practicable to separate it from the aggregate tension, and thus obtain a curve of a single progression representing the march of *atmospheric electricity*. The tables before us are strikingly confirmatory of the results obtained from the three years' observations. We see here, as there, the double progression most decided in the summer months; and the great tendency to a single progression in the winter is quite as conspicuous, if not more so than in those years. Upon the whole, the discussion of these seventeen months very strongly confirms the results already obtained.

One point only remains for our consideration; it is the annual period as exhibited by these numbers. It may probably be useful to particularize the annual period of each observation-epoch, and with this view the excess or defect as compared with the mean annual value of each epoch is given in the following table.

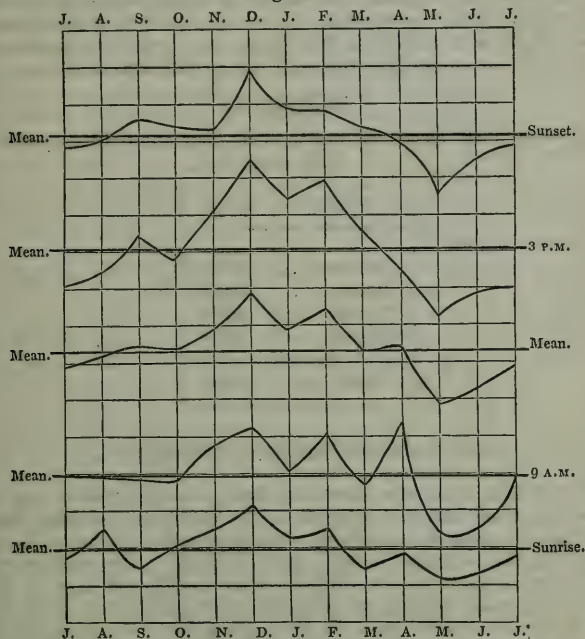
TABLE XC.

Excess or defect of the mean electrical tension (low, or below 60 div.) of each month in the year 1844, as compared with the mean annual value at each observation-epoch; also the excess or defect of the monthly mean tensions, as compared with the mean of the year.

	Sunrise.	9 A.M.	3 P.M.	Sunset.	Mean.
	div.	div.	div.	div.	div.
January . . .	+ 3·8	+ 1·2	+ 13·3	+ 7·3	+ 6·3
February . .	+ 6·9	+ 12·7	+ 19·0	+ 6·6	+ 12·0
March	— 5·4	— 1·7	+ 5·2	+ 1·5	+ 0·3
April	— 0·8	+ 15·6	— 6·3	— 2·1	+ 1·3
May	— 8·1	— 15·7	— 18·6	— 15·1	— 14·1
June	— 6·5	— 14·0	— 12·5	— 6·8	— 10·0
July	— 1·9	— 0·4	— 9·8	— 3·1	— 3·8
August	+ 6·6	— 0·8	— 6·8	— 1·3	— 0·6
September . .	— 4·7	— 1·2	+ 4·5	+ 4·9	+ 0·9
October	+ 1·8	— 1·2	— 2·3	+ 2·2	+ 0·1
November . .	+ 6·9	+ 8·6	+ 10·3	+ 1·5	+ 6·8
December . .	+ 11·9	+ 13·5	+ 25·3	+ 18·3	+ 16·4

In this table the depression of the summer readings below and elevation of the winter readings above the mean line at each epoch are very apparent. The differences however between the annual periods at each epoch and their approach to, or departure from, the mean is rendered more perceptible to the eye by the annexed curves (fig. 18).

Fig. 18.



Curves exhibiting the annual periods of the electrical tension (low) at the four observation-epochs for the year 1844.

It will be apparent from these curves that the *forenoon* and *afternoon* movements upon the annual period are strikingly different. There is considerable agreement between the curves of sunrise and 9 A.M., and also between those of 3 P.M. and sunset; the four curves forming two pairs, each pair presenting many features in common. The greatest difference between the curves is noticed at 9 A.M. and 3 P.M. There is more accordance between the sunrise and sunset curves, the principal difference occurring in the *opposite* movements in September. The sunrise curve is evidently modified by the movements at 9 A.M., as is that of sunset by the movements at 3 P.M. From this we may probably infer that the movements in the middle of the day, at least between 9 A.M. and 3 P.M., are much more irregular even in the low tensions than at any other period. The range (see Table XCI.), which is lowest at sunrise, increases rapidly between 9 A.M. and 3 P.M. the maximum; and this circumstance, taken in connexion with the cloudiness of the atmosphere (a subject to which we shall have occasion particularly to refer when treating on negative electricity), strongly indicates that the irregularity of movement in the middle of the day is more or less connected with the dis-

turbing influences of clouds. The curve of cloudiness, as deduced from six years' observations at Greenwich, will be found on page 198. The approximation towards agreement in the sunrise and sunset curve is greatly in accordance with the phenomena already noticed in the same curves from five years' observations (see page 162); and the differences observable between those for the year 1844 strongly confirm the remark that has been offered, viz. that a series of five years' observations at least is necessary to eliminate the effects of irregular movements. It may be remarked, in immediate connexion with the curves of 1844, that the sunrise curve is much more in accordance with the sunset than that at 9 A.M. is with that at 3 P.M.

TABLE XCI.

Annual range of the electrical tension at the four observation-epochs in the year 1844.

Epoch.	Range.
	div.
Sunrise. . . .	20·0
9 a.m.	31·3
3 p.m.	43·9
Sunset	33·4
Mean	30·5

PART II.—NEGATIVE ELECTRICITY.

The exhibition of negative electricity being confined within very narrow limits as compared with that of positive—the number of readings being extremely few—renders it exceedingly doubtful whether we can at all hope to find anything like a diurnal period manifested by it. The number of readings in the three years 1845 to 1847 amounted to 324, and it is not difficult to obtain the mean reading at each observation-hour from these records. In the seventeen months prior to 1845, great care was manifested in observing every and even the minutest change in the *kind* of electricity with which the conductor was charged. Not a shower appears to have occurred but it was minutely watched, the rapidity and extent of the changes assiduously observed, and the length of the sparks carefully measured; the whole being accompanied by notices of the weather at the time which appear to possess great accuracy of detail. As however the extremes of the charges are usually set down in some cases at the times they occurred, in others in a more general manner and *between certain epochs*, and not at such regular intervals, except on certain occasions, as would be useful in discussing them with a view to determine a diurnal period; such discussion, with regard to the negative observations previous to 1845, has not been attempted; but they have been carefully arranged in Table XCII. under the heads of "Limits of Time," "Extremes of Charge," "Maximum Length of Spark," and accompanying "Weather and Remarks." Under the last head are included the state of the weather with remarks by the observer at Kew; the clouds or other phenomena (likely to illustrate the Kew observations) recorded at or near the same epochs at the Royal Observatory, Greenwich, and occasional remarks by the writer. All remarks having reference to the Greenwich Observatory are placed within brackets.

In the succeeding table, the great majority of instances in which negative electricity has been exhibited, are characterized by *two* very interesting features. At Kew one of these features has been the *falling of rain*, in most instances *heavy*; and the other the great probability, from the almost constant record, at or near the same epochs at the Royal Observatory, Greenwich, of *cirro-stratus*, and occasionally *cumulo-stratus*, that these clouds have more or less not only accompanied, but contributed their quota to, the development of the electricity observed. On numerous occasions, *cirro-stratus* has been observed at Greenwich without the electrical instruments having been affected, and from this we may with great truth infer that *cirro-stratus* in its *ordinary* action does not occasion a disturbance of the regular diurnal march of the electrical tension. Most probably it is only when the conditions exist for the precipitation of rain, especially when the rain is formed very rapidly and in great quantities, that the electrical condition of the cloud is disturbed, and the conductor affected negatively. From the great constancy of the phænomena during a period of seventeen months, we are inclined to consider that to a certain extent they illustrate the remark relative to the *production of lightning by rain*, which occurs in the Report of the Committee of Physics, including Meteorology, approved by the President and Council of the Royal Society, pages 46 and 47. In speaking of thunder-storms, the writer, in alluding to one point to which the Committee wished some attention to be paid, says,—“It is the sudden gush of rain which is almost sure to succeed a violent detonation immediately over-head. Is this rain a *cause* or a *consequence* of the electric discharge? Opinion would seem to lean to the latter side, or rather, we are not aware that the former has been maintained or even suggested; yet it is very defensible. In the sudden agglomeration of many minute and feebly electrified globules into one rain-drop, the quantity of electricity is increased in a greater proportion than the surface over which (according to the laws of electric distribution) it is spread. Its tension therefore is increased, and may attain the point when it is capable of separating from the *drop* to seek the surface of the cloud, or of the newly formed descending body of rain, which under such circumstances, and with electricity of such a tension, may be regarded as a conducting medium. Arrived at this surface, the tension for the same reason becomes enormous, and a flash escapes.” In immediate reference to this remark, we apprehend the observations do not so much indicate the *actual electric tension of the rain falling on the conductor*, as the effect on the conductor of the *electric disturbance occasioned by the production of the rain*; this disturbance principally influencing the cloud from which the rain is precipitated, and through the cloud influencing the earth and bodies in its immediate neighbourhood. We shall have occasion again to refer to this subject in the *Notes* that are sub-joined.

The exceptions to the general fact of heavy rain falling when the conductor has been negatively electrified are rare; only ten instances are recorded during the seventeen months; they are given in Table XCIII. p. 185; some of them are extremely interesting, and are calculated to throw great light on the subject to which we have just alluded: we shall notice them in their proper places in the *Notes* that follow.

TABLE XCII.

Observations of Negative Electricity made at the Observatory of the British Association at Kew, between Aug. 1, 1843, and December 31, 1844.

Date.	Observed limits of time.		Observed extremes of Charge.	Max. length of Spark.	Weather and Remarks.
	h m	h m			
1843. Aug. 2.	1 30 p.m.	1 45 p.m.	N.	During a shower which began about 1 ^h 30 ^m p.m. the charge was negative for about 15 minutes; it changed to positive when the rain ceased. Showery from 10 a.m. and during the day. [Cirro-stratus and dark scud at Greenwich.—W. R. B.]
3.	11 0 a.m.	5 0 p.m.	P. 50 ^m N.	From 11 a.m. to 5 p.m. a continuation of heavy showers of rain at short intervals, the charge changing continually and rapidly during each shower. The negative charge 50 ^m was registered at 3 p.m. [Cumulo-stratus, cirro-stratus, nimbus and scud at Greenwich: a violent squall of rain from 9 ^h 50 ^m a.m. to 9 ^h 57 ^m a.m. which affected the galvanometer.—W. R. B.]
4.	0 45 p.m.	4 0 p.m.	90 ^m P.	N. 0-950	From about 0 ^h 45 ^m p.m. to 4 ^h 0 ^m p.m. continual heavy showers, during which the charges rose very high and changed rapidly from positive to negative. * * * Thunder was accompanied by a discharge which resembled a the afternoon and some lightning seen. One flash was accompanied by a discharge which resembled a luminous galvanic current; it lasted about half a second and was more than half an inch long ^(a) .
8.	8 0 p.m.	8 30 p.m.	40 ^m N.	At about 8 p.m. some heavy drops of rain fell and the conductor became negative; it changed to positive about 8 ^h 30 ^m p.m. [Cirro-stratus and scud at Greenwich.—W. R. B.]
15.	4 20 p.m.	9 30 p.m.	30 ^m N.	This period commenced with a thick mist to windward (E. at 3 p.m.), Volta 125 div. N. Between 6 ^h 45 ^m p.m. and 8 ^h 45 ^m p.m. several claps of thunder and flashes of lightning were heard and seen. From 9 p.m. to 9 ^h 30 ^m p.m. heavy rain fell: the negative charge 30 ^m and the spark were registered at 9, and at the termination of the rain the charge became positive ^(b) .
16.	6 0 a.m.	8 15 a.m.	30 ^m N.	At about 6 a.m. a heavy rain began, accompanied by thunder, lightning, and continual rapid changes of the charge from positive to negative. The negative charge 30 ^m was registered at 7 ^h 16 ^m a.m. Immediately before a clap of thunder (? time) a continued stream was observed to flow from the conductor to the discharger about .5 inch long and lasting about 2 seconds. [This appears to have been a continuation of the same kind of weather observed on the previous evening.—W. R. B.] [At Greenwich cirro-stratus and scud with a low rumbling of thunder in the S.W. were registered at 7 ^h 20 ^m a.m.; it was first heard at 7 ^h 5 ^m a.m. and passed round by the N.W.—W. R. B.]
22.	5 15 p.m.	5 30 p.m.	100 ^m N.	Heavy rain from 11 a.m. to 5 ^h 30 ^m p.m. The charge during this rain was positive until the last quarter of an hour. It became again positive on the ceasing of the rain. [Cirro-stratus and scud at Greenwich.—W. R. B.]
23.	8 30 p.m.	9 15 p.m.	10 ^m P.	N.	Heavy rain from 3 p.m. The charge was positive but disturbed during this rain; the reading 10 ^m P. was registered at 3 ^h 15 ^m p.m. shortly after its commencement. The negative electricity did not appear until 8 ^h 30 ^m p.m. [The largest amount of rain recorded in 24 hours at Greenwich between October 1840 and April 1845; it was rather above 2 inches.—W. R. B.]

Sept. 10.	2 10 p.m.	2 47 p.m.	40°P.	45°N.	0-400	Between 2 ^h 10 ^m P.M. and 2 ^h 47 ^m P.M. a thunder-cloud passed over the observatory from S.W. to N.E., during which passage four different changes in kind of charge took place. The tensions registered were the highest observed; they gradually diminished until the positive signs of serene weather returned (°).
Oct. 2.	11 0 a.m.	11 10 a.m.	N.	This 10 minutes of negative charge (value not recorded) occurred during a light rain from 10 A.M. to 1 ^h 30 ^m P.M.; it was accompanied by heavy rain, so that this heavy rain might be considered as a distinct shower from the general light rain, the signs becoming positive upon its ceasing (°).
6.	11 30 a.m.	noon.	75°N.	During a heavy shower. Other showers occurred during the afternoon, but although negative electricity was not exhibited, disturbance was manifested. [Cirro-stratus at Greenwich.—W. R. B.]
7.	0 30 p.m.	4 0 p.m.	10°P.	30°N.	0-200	During a shower in which the changes were frequent. [Cirro-stratus, cumulus, cumulo-stratus and scud registered at Greenwich.—W. R. B.]
9.	8 45 a.m.	9 0 a.m.	25°P.	15°N.	Heavy shower.
9.	5 20 p.m.	6 45 p.m.	25°P.	20°N.	0-200	Weather between the showers dull and cloudy. [Cirro-stratus and scud at Greenwich.—W. R. B.]
11.	11 0 a.m.	11 15 a.m.	11°N.	Several very heavy showers between 11 A.M. and 4 P.M. A negative charge characterized the first only for about a quarter of an hour. [Cirro-stratus and heavy rain at Greenwich.—W. R. B.]
12.	11 0 a.m.	2 45 p.m.	25°P.	30°N.	This period is divided into two portions: one of 2 ^h 15 ^m duration, characterized by heavy rain; the other 1 ^h 30 ^m by fine weather accompanied with sunshine. [I subjoin in the Notes the original entry.—W. R. B.] (°)
21.	1 15 p.m.	1 45 p.m.	5°N.	During a heavy shower. [Cirro-stratus and scud at Greenwich.—W. R. B.]
28.	3 0 p.m.	3 10 p.m.	30°N.	0-200	During a heavy shower of hail and rain (†).
31.	11 0 a.m.	8 0 p.m.	17°N.	0-150	A general heavy rain began about 9 ^h 30 ^m A.M. which continued during the whole day and evening; the negative charge commenced at 11 A.M. and continued the whole day and evening. [Cirro-stratus and scud registered at Greenwich.—W. R. B.]
Nov. 8.	0 15 p.m.	0 40 p.m.	55°P.	40°N.	0-400	During a shower. [Cirro-stratus and scud were registered at Greenwich at 1 ^h 20 ^m P.M., and a squall of hail was observed there from 0 ^h 35 ^m P.M. to 0 ^h 45 ^m , during which the galvanometer needle deviated to 45° towards A.—W. R. B.]
18.	2 0 p.m.	2 20 p.m.	40°P.?	35°N.	0-250	During a heavy shower of rain and hail. [This shower did not occur at Greenwich, but at the observations before and after its occurrence at Kew, large white cumuli were observed at Greenwich, at first in every direction and afterwards in the W. S. and E.—W. R. B.]
24.	0 0 m.	1 30 p.m.	6°N.	Heavy rain and fog during the first half-hour; the negative charge continued an hour after the rain had ceased, and half an hour after the maximum charge. [Cirro-stratus registered at Greenwich from 5 ^h 20 ^m A.M. to 1 ^h 20 ^m P.M.: rain occurred till within a few minutes of 1 ^h 20 ^m P.M.: a heavy rain is registered from 2 ^h 40 ^m P.M. to 3 ^h 5 ^m P.M., which did not appear to have affected the electrical instruments. Cirro-stratus is not named.—W. R. B.]
25.	9 0 a.m.	10 30 a.m.	20°P.	25°N.	0-150	Heavy rain. [Cirro-stratus registered at Greenwich.—W. R. B.]
Dec. 31.	3 0 p.m.	4 30 p.m.	75°P.	100°N.	Heavy rain. The negative charge commenced about sunset. [Cirro-stratus and scud registered this day at Greenwich.—W. R. B.]
1844.						
Jan. 1.	sunrise.	9 0 a.m.	20°P.	16°N.	Snow and rain. [Cirro-stratus and scud registered at Greenwich.—W. R. B.]

TABLE XCII. (*continued*).

Date.	Observed limits of time.		Observed extremes of Charge.	Max. length of Spark.	Weather and Remarks.
	h m	h m			
1844.				in.	
Jan. 5.	sunset.	4 15 p.m.	12° P.	5 in.	Heavy rain. [Cirro-stratus and scud registered at Greenwich.—W. R. B.]
5.	6 0 p.m.	6 45 p.m.	15° P.	10 in.	Heavy rain. [Cirro-stratus and scud registered at Greenwich.—W. R. B.]
13.	8 0 a.m.	8 20 a.m.	7° P.	19 in.	Heavy shower. [Negative electricity was registered at Greenwich between 11 p.m. of the 12th and 6 ^h 7 ^m A.M. of the 13th. Maximum tension 40 ^s single gold leaf of dry pile apparatus.—W. R. B.]
23.	9 0 a.m.	9 30 a.m.	6 in.	A shower. [Cirro-stratus and thick fog registered at Greenwich at 9 ^h 20 ^m A.M., also positive electricity between 9 A.M. and 9 P.M. Max. tension 33° of dry pile.—W. R. B.]
30.	7 30 p.m.	Heavy rain. [Cirro-stratus and scud at Greenwich.—W. R. B.]
31.	8 45 a.m.	45° P.	80° N.	Heavy storm of snow and hail.
31.	0 0 m.	0 20 p.m.	50° P.	90 in.	Storm of snow and hail.
31.	1 40 p.m.	2 0 p.m.	60° P.	90 in.	Heavy shower of rain and hail (5).
Feb. 7.	4 30 p.m.	5 20 p.m.	20 in.	0-150	Heavy rain. [Cirro-stratus registered at Greenwich.—W. R. B.]
7.	7 0 p.m.	11 0 p.m.	16° P.	10 in.	Heavy rain. [Cirro-stratus registered at Greenwich between 3 ^h 20 ^m A.M. and 9 ^h 20 ^m P.M.—W. R. B.]
8.	4 0 p.m.	4 15 p.m.	35° P.	20 in.	Heavy shower of rain and hail. [At 5 ^h 20 ^m , more than an hour later, cirro-stratus and scud was registered at Greenwich, but at 3 ^h 50 ^m P.M. a squall of rain and hail was observed; the electrometer not affected, except by a sudden but slight start in the pith-balls shortly before the squall.—W. R. B.]
9.	4 30 p.m.	5 0 p.m.	60° P.	70 in.	Heavy storm of rain and snow. [Cirro-stratus and scud registered at Greenwich at 5 ^h 20 ^m P.M. At 5 ^h 5 ^m a shower of sleet and snow fell which ceased at 5 ^h 20 ^m P.M.—W. R. B.] ^(h)
19.	11 0 a.m.	11 15 a.m.	12 in.	Heavy rain. [Cirro-stratus and scud registered at Greenwich from 11 ^h 20 ^m A.M. to 9 ^h 20 ^m P.M.; at 10 ^h 10 ^m A.M. a squall of heavy rain commenced; it continued until 11 ^h 45 ^m A.M., the electricity being negative during the last 45 minutes. Maximum tension 40 div. Volta (2).—W. R. B.]
21.	11 30 a.m.	95° P.	20 in.	Light rain with intermissions. [Cirro-stratus and scud registered at Greenwich from 5 ^h 20 ^m A.M.—W. R. B.]
23.	5 0 p.m.	20 in.	Light rain and snow; heavy rain. [Cirro-stratus registered at Greenwich from 3 ^h 20 ^m P.M. to 7 ^h 20 ^m P.M. with thin rain, succeeded by squalls of heavy rain. Negative electricity from 4 ^h 50 ^m P.M. to 6 ^h 12 ^m P.M. Max. tension 45 div. Volta (2) at 5 ^h 33 ^m P.M. Max. length of spark 0.10 inch.—W. R. B.]
25.	2 40 p.m.	2 56 p.m.	60° P.	25 in.	Heavy rain. [Cirro-stratus and scud registered this day at Greenwich.—W. R. B.]
26.	10 45 a.m.	11 0 a.m.	55° P.	25 in.	Heavy shower. [Cirro-stratus and scud 9 ^h 20 ^m A.M. at Greenwich.—W. R. B.]
26.	0 5 p.m.	0 20 p.m.	5° P.	15 in.	Heavy shower. [Before 1 ^h 12 ^m P.M. a large cumulo-stratus approached the zenith of Greenwich, and all the instruments became at once affected; at 1 ^h 12 ^m a few heavy drops of rain fell. The electricity exhibited was negative. Max. tension 100 div. Volta (2). Sparks 0.15 inch in length. 1 in 10 seconds occurred at 1 ^h 22 ^m P.M.—W. R. B.] ⁽ⁱ⁾
29.	7 0 p.m.	10 0 p.m.	90° P.	20 in.	Light and heavy rain. [Cirro-stratus registered at Greenwich at 7 ^h 20 ^m P.M. At 9 ^h 20 ^m heavy rain. Between 9 ^h 55 ^m and 10 ^h 20 ^m P.M. negative electricity. Max. tension 23 div. Volta (1). The observer adds this note: "Rain is falling; during the continuance of previous heavy rain no effect was noticed."—W. R. B.]

Mar. 2.	4 15 p.m.	4 30 p.m.	45°P.	35°N.	0-400	Heavy shower. [The clouds registered at Greenwich about this time were cumuli and scud.—W. R. B.]
3.	2 30 p.m.	2 35 p.m.	60°P.	60°N.	0-500	During the passage of a very heavy cloud from which fell a little hail. [This was most probably a cumulo-stratus, that cloud with nimbi and scud being observed at Greenwich in the forenoon.—W. R. B.]
4.	sunrise.	5 0 p.m.	35°N.	0-300	Heavy and light rain. [Cirro-stratus was registered at Greenwich from 5 ^h 20 ^m A.M. to 11 ^h 20 ^m . Between 6 ^h 25 ^m A.M. and 8 A.M. negative electricity was observed. Max. tension Volta (2) 150 div., rain falling heavily at the time. Also between 11 ^h 20 ^m A.M. and 1 P.M. max. tension Volta (2) 50 div., rain falling heavily.—W. R. B.]
10.	sunrise.	1 0 p.m.	10°N.	0-100	Heavy rain, during which the charge was always negative.
13.	2 0 p.m.	2 20 p.m.	60°P.	35°N.	0-600	Heavy shower of rain and hail. [At 3 ^h 20 ^m P.M. dense cumulo-stratus covered the sky in the N. and N.E. at Greenwich.—W. R. B.]
14.	7 0 p.m.	9 0 p.m.	20°N.	0-400	General heavy rain. [Cirro-stratus registered at Greenwich from 3 ^h 20 ^m A.M. Negative electricity was also observed between 10 A.M. and 6 ^h 26 ^m P.M. Max. tension Volta (2) 150 div., and sparks occurred 0-07 inch in length, 1 in 25 seconds, and 2 in 101 seconds.—W. R. B.]
15.	7 0 a.m.	7 30 a.m.	10°N.	0-100	Heavy rain. [Cirro-stratus at Greenwich.—W. R. B.]
20.	1 0 p.m.	1 30 p.m.	20°P.	45°N.	0-400	Heavy shower. [Cirro-stratus and scud with squalls of rain at Greenwich. At 0 ^h 48 ^m P.M. to 0 ^h 50 ^m P.M. a squall occurred, in which negative electricity, tension 15 div. Volta (2), was exhibited; at 1 ^h 23 ^m P.M. dark clouds came up from the W.; at 1 ^h 32 ^m P.M. heavy rain commenced falling, after which a squall occurred; the electricity was pos. and neg. between 1 ^h 23 ^m and 1 ^h 58 ^m , tension 100 Volta (2); sparks purple and white, 0-16 inch in length.—W. R. B.]
Apr. 5.	9 0 a.m.	15°N.	Heavy rain. [Nimbi and cirro-stratus at Greenwich at 9 ^h 20 ^m A.M.—W. R. B.]
5.	0 20 p.m.	0 40 p.m.	19°N.	0-175	Heavy shower of rain.
13.	3 0 p.m.	25°N.	0-200	Heavy shower of rain. [Cirro-stratus and scud at Greenwich.—W. R. B.]
14.	5 0 p.m.	5 20 p.m.	10°P.	35°N.	0-900	Heavy shower of rain. [Cirro-stratus and scud at Greenwich.—W. R. B.]
May 4.	11 30 a.m.	65°N.	Light rain. [Cirro-stratus and scud at Greenwich.—W. R. B.]
17.	11 0 a.m.	35°N.	Very dull and overcast from 11 A.M. to 3 P.M.; a little rain fell at noon which charged the rod negatively. [Cirro-stratus and scud at Greenwich.—W. R. B.]
18.	11 15 a.m.	10°P.	22°N.	0-100	A shower of rain. [At 10 ^h 50 ^m A.M. there were a few fleecy clouds in the zenith of Greenwich; the remainder of the sky was covered with cirro-stratus; small quantities of sleet fell shortly before noon, after which it was generally clear. Positive and negative electricity exhibited between 10 ^h 50 ^m A.M. and 4 ^h 30 ^m P.M., max. tension 200 div. Volta (2); numerous sparks, max. length 0-28 inch.—W. R. B.]
18.	1 0 p.m.	55°P.	30°N.	0-450	Heavy shower of rain and hail; the sparks were obtained from positive electricity.
18.	2 10 p.m.	50°P.	Heavy shower of rain (k).
27.	10 0 a.m.	10 15 a.m.	20°N.	Heavy shower of rain. [During the continuance of these observations cirro-stratus, scud, and cumulo-stratus were registered at Greenwich; electrical changes were observed
27.	10 30 a.m.	10 40 a.m.	30°N.	between 11 ^h 0 ^m A.M. and 4 ^h 5 ^m P.M., max. tension 100 div. Volta (2), hail and
27.	11 5 a.m.	11 15 a.m.	35°P.	40°N.	0-400	Heavy shower of rain. } rain falling. It will be remarked that the phenomena were more or less si-
27.	2 25 p.m.	2 45 p.m.	55°N.	Heavy shower of rain. } milar at both stations.—W. R. B.]
27.	4 30 p.m.	4 45 p.m.	80°N.	1-200	Heavy shower of rain. }
29.	10 0 p.m.	90°N.	Heavy rain. [Cirro-stratus with continued rain at Greenwich.—W. R. B.]

TABLE XCII. (*continued*).

Date.	Observed limits of time.		Observed extremes of Charge.	Max. length of Spark.	Weather and Remarks.
1844.					
June 10.	h m	h m			Heavy shower (¹). Thunder-storm (^m). A light shower of rain. [Cirro-stratus and scud registered at Greenwich; also at 5 ^h 20 ^m p.m. a shower of hail which lasted but three minutes; heavy clouds were observed in every direction.—W. R. B.]
18.	3 40 p.m.	5 30 p.m.	90°P. 65°P.	55°N. 60°N.	General rain, occasionally heavy, during which the charge varied both in kind and intensity several times. [Cirro-stratus registered at Greenwich nearly the whole day, and rain from 5 ^h 35 ^m A.M. of the 25th until 3 ^h 20 ^m A.M. of the 26th. Electrical changes occurred between 9 ^h 10 ^m A.M. and 11 ^h 25 ^m A.M. of the 25th, maximum tension 20° of Henley; sparks very numerous, in one instance in a valley, but not longer at any time than 0.10 inch; on one occasion 15 occurred in 8 seconds; the electricity was positive till 9 ^h 23 ^m A.M., when it suddenly changed to negative; at 9 ^h 36 ^m a rumbling of thunder was heard in the S.W.; at 9 ^h 55 ^m there was a slight galvanic current, the point of the needle moving to- wards B.—W. R. B.]
19.	5 15 p.m.	5 25 p.m.	9°N.	Light rain with intermissions. Thunder-storm (ⁿ). Heavy rain.
25.	8 0 a.m.	7 0 p.m.	49°P.	29°N.	Heavy shower of rain. [Cumulo-stratus and dark heavy scud very near the earth, registered at Green- wich.—W. R. B.]
27.	5 0 a.m.	11 0 a.m.	40°P.	30°N.	<i>Dull and cloudy.</i>
July 1.	5 30 p.m.	7 50 p.m.	60°P.	70°N.	Heavy rain. [Cirro-stratus and scud at Greenwich.—W. R. B.]
2.	9 0 a.m.	77°5°N.	Heavy and light rain with intermission; the maximum charge occurred at 3 ^h 45 ^m p.m. just after heavy rain, but when it had ceased. [Cirro-stratus and scud at Greenwich; heavy rain between 3 ^h 55 ^m p.m. and 5 p.m.—W. R. B.] (^o)
2.	7 30 p.m.	7 45 p.m.	N.	<i>Dull and cloudy.</i>
2.	sunset.	15°N.	Light rain.
4.	3 0 p.m.	3 5 p.m.	20°P.	15°N.	<i>Fine but cloudy; charge high.</i>
5.	3 0 p.m.	5 0 p.m.	23°N.	Heavy and lighter rain. [Cirro-stratus, cumulo-stratus, nimbi, and dark scud registered at Green- wich from 1 ^h 20 ^m p.m., also negative electricity from 5 ^h 20 ^m p.m. to 5 ^h 25 ^m p.m.; maximum tension Volta (1) 12 div.: negative electricity had been previously observed between 11 ^h 12 ^m A.M. and 1 ^h 4 ^m P.M.; max. tension Volta (2) 50 div.; max. length of sparks 0.11 inch, 20 in 20 seconds.—W. R. B.]
6.	4 0 p.m.	5 0 p.m.	3°N.	10°N.	General rain; the negative charge was registered at 2 p.m. [Dark cumuli, cirro-stratus, and scud registered at Greenwich just previous to the commencement of the rain, which continued almost with- out intermission until 9 ^h 20 ^m p.m.—W. R. B.]
12.	1 25 p.m.	1 30 p.m.	35°N.	Light and heavy rain. [Cirro-stratus and scud at Greenwich.—W. R. B.]
12.	1 35 p.m.	2 0 p.m.	N.	Light rain. [Cumuli and cirri registered at Greenwich.—W. R. B.]
12.	4 15 p.m.	4 25 p.m.	55°P.	40°N.	<i>Distant rain and storm; sunshine at Kew.</i> [There are no indications of this rain having been observed at Greenwich.—W. R. B.]
13.	2 0 p.m.	9 30 p.m.	35°P.	5°N.	
16.	11 0 a.m.	0 50 p.m.	90°P.	40°N.	
18.	4 0 p.m.	5 0 p.m.	45°P.	60°N.	
18.	6 20 p.m.	6 30 p.m.	50°N.	

July 19. 11	3 a.m.	45°P.	3 ^h N.	0-400	Heavy shower. At the beginning of this shower the Henley rose to 45° pos. in three minutes, afterwards Volta was at 60° neg., in three more minutes it rose to 3° Henley neg., then declined, and the charge changed to positive as usual. [Cumulo-stratus and haze, registered at Greenwich at 11 ^h 20 ^m A.M.—W. R. B.]
19.	3 20 p.m.	4 47 p.m.	65°P.	60°N.	Violent storm of thunder, hail and rain; it was considered to be the most violent that had occurred. [The phenomena, which are recorded at page 134 of the volume for 1844, Storm-paper No. 3, may be divided into two portions: one characterized by the discharges and <i>hail</i> , duration 17 minutes; the other by heavy and light rain, duration 1 hour; mean tension of the first period 53° Henley, mean tension of the second 42° of Henley. We have here another instance of higher tensions accompanying discharges (see notes on Table XCII.). The production of the hail in the apparent axis of the storm is not without interest. At Greenwich cirro-stratus and scud, with heavy cumulo-strati, from which, at intervals, emanated a rumbling of thunder, were registered at 3 ^h 20 ^m P.M., and from 1 ^h 59 ^m P.M. to 5 ^h 24 ^m negative electricity was observed; maximum tension 200 div. of Volta (2), max. length of spark 0-10, rain generally falling during the time; thunder occasionally heard; at 3 ^h 34 ^m the rain ceased.—W. R. B.]
30.	3 25 p.m.	3 56 p.m.	10°P.	20°N.	0-300	Heavy rain. [The details of this shower may be found on page 134 of the volume for 1844. Cirro-stratus and scud, registered at Greenwich, also numerous heavy showers of rain between 3 ^h 20 ^m P.M. and 5 ^h 20 ^m P.M.—W. R. B.]
31.	3 30 p.m.	4 4 p.m.	40°P.	40°N.	0-400	Heavy and light rain. [Cirro-stratus at Greenwich.—W. R. B.]
Aug. 1. 3	0 p.m.	5°N.	General rain.
2.	1 45 p.m.	4 0 p.m.	60°P.	55°N.	0-700	Two heavy showers within this period. [Nimbi, cumulo-strati, and scud registered at Greenwich: the observer describes the sky as very wild; frequent sparks were observed of 0-13 inch; electricity positive and negative.—W. R. B.]
3.	7 35 a.m.	55°P.	70°N.	0-800	Heavy shower of rain. [Cirro-stratus and scud at Greenwich, also negative electricity between 11 ^h 0 ^m A.M. and 11 ^h 14 ^m A.M.; max. tension 3° of Henley; sparks 0-10 inch; a sudden change of electricity; the charge was again negative at 1 ^h 53 ^m P.M. to 2 ^h 0 ^m P.M.; max. tension 7° of Henley; max. length of spark 0-10 inch, occurred at 1 ^h 56 ^m , 40 in 20 seconds.—W. R. B.]
6.	4 0 p.m.	5°N.	A heavy dark cloud passed to the eastward. [At Greenwich nimbi near the horizon, and rather dense cumuli of a loose texture scattered in other parts of the sky were registered at 3 ^h 20 ^m P.M.—W. R. B.]
7. 11	0 a.m.	0 20 p.m.	75°P.	60°N.	1-700	Rain. [Nimbi, cumuli, and scud at Greenwich. From 11 ^h 8 ^m A.M. to 0 ^h 47 ^m P.M. negative electricity was observed; max. tension 12° of Henley; numerous sparks; max. length 0-18 inch; heavy clouds passing over the zenith.—W. R. B.]
8.	1 26 p.m.	50°N.	0-500	<i>Fine rain at a distance; sunshine.</i> [Cumuli, cumulo-strati, and scud at Greenwich; a shower of rain at 1 ^h 35 ^m P.M.; electricity during this shower negative; maximum tension 100 div. Volta (2).—W. R. B.] (P)
11.	6 0 p.m.	50°N.	General heavy rain. [Heavy rain at Greenwich from 7 P.M.—W. R. B.]
12. 10	22 a.m.	15°N.	0-150	Light rain. [At Greenwich the rain was falling heavily at 11 ^h 30 ^m A.M.; the electricity was negative; max. tension 10 div. Volta (2).—W. R. B.]
14.	9 0 a.m.	4 0 p.m.	10°P.	62-5°N.	0-500	General light rain with intermissions. [At Greenwich heavy showers, cirro-stratus, and scud registered. The electrical instruments were affected between 11 ^h 30 ^m A.M. and 11 ^h 53 ^m , both negatively and positively; max. tension 7° of Henley; sparks abundant, 0-10 inch; at 11 ^h 45 ^m A.M. the electricity changed suddenly from positive to negative.—W. R. B.]

Date.	Observed limits of time.		Observed extremes of Charge.		Max. length of Spark.	Weather and Remarks.
	h	m	h	m	in.	
1844. Aug. 15.	4	0 p.m.	4 ^h N.	Light rain. [At Greenwich we have the following record :—" Since 3 ^h 15 ^m Göttingen time, 2 ^h 35 ^m Greenwich time, there have been several dashing showers of rain ; scud has passed quickly from the N. ; at present (3 ^h 20 ^m p.m.) it is squally and the sky is covered with so dark a cloud as to cause a great gloom." " Immediately after the observation at 4 ^h (3 ^h 20 ^m p.m.), a heavy shower of rain fell and continued for about fifteen minutes ; since that time no rain has fallen." From 3 ^h 50 ^m to 4 ^h 35 ^m p.m., a large cloud in the zenith, rain falling, gloomy ; there were several changes from negative to positive electricity ; max. tension 40 div. Volta (2).—W. R. B.] <i>Dull and cloudy.</i> [Cirro-stratus and scud at Greenwich.—W. R. B.] Heavy rain ; several flashes of lightning seen, and distant thunder heard. [At Greenwich we have the following record :—" 5 ^h 20 ^m p.m. ; the sky is nearly covered with thin cirro-stratus ; after this time the sky became overcast, and at 6 ^h 30 ^m p.m. the clouds in the S. became black, and shortly afterwards several flashes of lightning emanated from them ; lightning continued at intervals of about two minutes till 8 ^h 15 ^m ; at that time thunder was heard, and lightning continued as before till midnight ; heavy rain commenced falling about 8 p.m." The electrical instruments were not affected. Lightning had been prevalent more or less during the three previous nights. It would appear that the lightning was either not observed or not visible at Kew. On this evening it appeared to have been more prevalent at Greenwich than at Kew.—W. R. B.] <i>Fine but cloudy, with sunshine.</i> [At Greenwich cumulo-stratus towards the N., and cirro-stratus and scud in different directions.—W. R. B.] Heavy and light rain. [Cirro-stratus and scud at Greenwich.—W. R. B.] Heavy shower of rain. [Cumulo-strati and scud had been previously registered at Greenwich.—W. R. B.] Heavy shower of rain. [Cirro-stratus and scud at Greenwich, heavy rain falling between 11 ^h 43 ^m A.M. and 11 ^h 46 ^m A.M. ; at 11 ^h 44 ^m A.M. there was a sudden change from negative to positive electricity ; max. tension 40 div. Volta (2).—W. R. B.] Heavy rain. [Cirro-stratus and scud at Greenwich.—W. R. B.] General heavy rain. [Cirro-stratus and scud with heavy rain at Greenwich. Negative electricity between 5 ^h 42 ^m p.m. and 6 ^h 50 ^m p.m., max. tension 80 div. Volta (2).—W. R. B.] General rain. [Overcast and heavy rain at Greenwich at 11 ^h 20 ^m A.M. ; at 10 ^h 33 ^m to 10 ^h 49 ^m , negative electricity ; max. tension 15 div. Volta (2).—W. R. B.] General heavy rain, during which the electricity was always negative. General rain. [Cirro-stratus and scud with heavy rain at Greenwich, also negative electricity between 11 ^h 30 ^m A.M. and 11 ^h 35 ^m A.M. ; tension 2 div. Volta (1).—W. R. B.] Light rain. Heavy shower of rain. Heavy shower of rain. [Cirro-stratus and scud at Greenwich.—W. R. B.] General heavy rain. Heavy rain. General heavy rain.
Sept. 5.	10	0 a.m.	5 ^h N.	
8.	9	0 p.m.	45 ^h N.	
17.	3	0 p.m.	4	0 p.m.	5 ^h N.	
18.	0	0 m.	2	0 p.m.	0-100
Oct. 13.	7	15 p.m.	7	45 p.m.	50 ^h N.	0-450
14.	11	5 a.m.	0	44 p.m.	40 ^h N.	0-400
16.	sunrise.	20 ^h N.
21.	sunset.	10	0 p.m.	50 ^h P.	20 ^h N.	0-175
24.	11	0 a.m.	10 ^h N.	0-100
24.	sunset.	9	30 p.m.	5 ^h N.
Nov. 2.	sunrise.	1	0 p.m.	30 ^h N.	0-300
3.	sunrise.	15 ^h N.
8.	0	45 p.m.	30 ^h N.	0-300
8.	3	0 p.m.	5 ^h N.
10.	1	0 p.m.	25 ^h N.	0-200
13.	sunrise.	9	0 a.m.	40 ^h N.
Dec. 29.	sunset.	55 ^h N.

TABLE XCIII.—Instances of negative electricity *without rain*.

Date.	Observed limits of time.		Observed extremes of charge.		Max. length of spark.
1843 and 1844.	h	m	h	m	in.
Sept. 10.	2	10 p.m.	2	47 p.m.	0.400
Oct. 12.	1	15 p.m.	2	45 p.m.	N.
Nov. 24.	0	30 p.m.	1	30 p.m.	6 ⁿ N.
July 2.	Sunset.		15 ^v N.
„ 6.	4	0 p.m.	5	0 p.m.	3 ^v N.
„ 12.	1	35 p.m.	2	0 p.m.	N*.
„ 18.	6	20 p.m.	6	30 p.m.	50 ⁿ N.
Aug. 8.	1	26 p.m.	50 ⁿ N.
Sept. 5.	10	0 a.m.	5 ⁿ N.
„ 17.	3	0 p.m.	4	0 p.m.	5 ⁿ N.

* Charge high.

Notes on TABLE XCII.

(^a) *August 4, 1843.*—This thunder-storm was observed at Greenwich: *cumulo-stratus* and *scud* were registered there at 3^h 20^m P.M. with occasional showers, soon after which the sky assumed a very stormy appearance, more particularly in the N. and N.W.; at 3^h 45^m P.M. a low muttering of distant thunder was heard from dark clouds in the N.W., and thunder has been heard at intervals to the present time, 5^h 20^m P.M.: at 4^h 40^m P.M. rain began to fall, and it has continued: at 4^h 0^m P.M. a fine double rainbow was visible in the E.N.E., and at 5^h 20^m P.M. another very perfect one, also double in the E.: at present, 5^h 20^m P.M., a large clear break is near the horizon in the W., and it is the only part of the sky which is not covered with a dense cirro-stratus. At 7^h 20^m large loose fragments of *scud* were passing from the S.W., the portions of the sky without cloud being remarkably clear; the rain which commenced at 4^h 40^m P.M. ceased at 5^h 30^m P.M.; the last clap of thunder was heard at 5^h 35^m; it proceeded from dark clouds in the E.: *no lightning was seen during the whole time.* The galvanometer was affected, the needle moving towards A.

(^b) *August 15, 1843.*—The thunder-storm was observed at Greenwich. At 3^h 20^m P.M. *cumuli* and *cumulo-strati* were seen; weather hazy. At 5^h 20^m P.M. the same clouds were registered, and the observer thus writes: “Deep mutterings of thunder are heard, proceeding from dark *cumulo-strati* towards the N.E.: the weather is unusually sultry for this time of the day; temperature now at its maximum.” At 7^h 20^m “massive *cumulo-strati* and *nimbi* in all directions: at 5^h 40^m P.M. a loud clap of thunder was heard from the S.E., and from that time to 6^h 10^m P.M. a constant succession of claps took place; no lightning was seen: between 5^h 50^m P.M. and 6^h 5^m P.M. the rain fell very heavily: distant thunder has been heard to the present time.” “At 9^h 20^m overcast: at 7^h 40^m a vivid flash of lightning was seen in the N.E. which was followed by many others, chiefly forked, and accompanied by a heavy rolling of thunder, all from the N.E.: at present distant thunder is heard, and occasionally faint flashes of lightning from the N.W.: during the time the storm was in the N.E. the zenith was clear.” Between 5^h 42^m P.M. and 6^h 12^m P.M., the galvanometer was affected; maximum deviation towards A 50° at 5^h 49^m 3^s, and towards B 65° at the same time. This, the greatest oscillation, occurred on the occasion of a loud clap of

thunder; numerous other oscillations occurred with thunder, and rain falling in torrents.

(c) *September 10, 1843.*—We have here a well-marked instance of the regular diurnal march being interrupted by the *passage of a cloud* in the immediate neighbourhood of the observatory. No rain appears to have fallen, yet the instruments were thrown into a state of oscillation, positive to negative, which gradually diminished as the cloud passed off; a spark or sparks 0·4 inch in length were registered. This cloud appears to have been a *cumulo-stratus*; for at Greenwich at 3 P.M. *cumulo-stratus* is registered, and during the succeeding twenty minutes a *very heavy shower of rain accompanied with thunder* is recorded. The electrical instruments were not affected.

(d) *October 2, 1843.*—The connexion in this instance between the *heavy rain* and *negative charge* is very apparent, and would, combined with the observation of September 10, greatly tend to refer the production of the charge to the particular cloud by the agency of which the rain was precipitated, rather than to the rain itself. Cirro-stratus was registered at Greenwich from 9^h 20^m A.M. to 5^h 20^m P.M. The violent squall of rain occurred there at 5 minutes before 11 A.M.

(e) *October 12, 1843.*—"From sunrise until about 11 A.M. dull and cloudy. At about 11 A.M. a heavy rain began and continued until about 1^h 15^m P.M. At its commencement Volta stood at 25° pos.: immediately afterwards the charge became negative, the maximum of which was 30° of Henley, and a negative state continued until about 2^h 45^m P.M. The positive charge then remained during the rest of the day. The negative state existed about 1 hour and 30 minutes after the rain had ceased; and the weather during this period was fine and accompanied with sunshine. The duration of the negative state of the conductor, viz. about 3 hours 45 minutes, from about 11 A.M. to 2^h 45^m P.M., one hour and a half of which time elapsed without rain, is I believe a rare occurrence, and one which I do not recollect to have observed in my former experiments.—[F. R.]"

The negative state of the conductor during the three half-hours is remarkable. It appears the sun was shining and the weather fine; but the register does not inform us whether clouds were present or not. On turning to the Greenwich observations we find rain recorded at 11^h 20^m A.M., and at 1^h 20^m P.M. thin rain falling; the rain appears to have ceased earlier than this at Kew. From 9^h 20^m A.M. until 7^h 20^m P.M. *cirro-stratus* was registered at Greenwich; and as this cloud frequently manifests itself in the form of a *thin but very extensive stratum*, it is not unlikely that it was the source of the negative charge observed.—[W. R. B.]

(f) *October 28, 1843.*—At 3^h 20^m P.M. this squall was observed at Greenwich *without the hail*. The observer thus writes: "At present there is a violent squall: the rain is falling in large drops: the sky is covered with a nimbus: a few minutes since a cumulo-stratus with coloured edges was in the west, and scud was passing quickly from the west with a fine blue sky between." The head of the galvanometer needle deviated towards A 5°.

(g) *January 31, 1844.*—The electrical phenomena of this day being particularly interesting, and well-marked both at Kew and Greenwich, we cannot do better than present the reader with the records at both observatories.

Kew.

First Storm.—At sunrise fine, but cloudy. At 8^h 45^m A.M. a heavy storm of snow and hail began, when Volta stood at 35^{div} pos. The charge immediately changed to neg., in the maximum of which charge the Henley vibrated above 90°, and a stream of fire one inch long flowed from the con-

ductor to the discharger for at least four or five minutes during the time that the storm was at its height. At about 9 the storm had ceased, when the charge returned to pos. maximum 45° of Henley.

Second Storm.—At noon another storm of snow and hail began, when Volta stood at $105^{\text{div.}}$ pos.; but the charge immediately changed to neg., and the Henley again vibrated above 90° , sparks $1\frac{5}{10}$ inch. The positive maximum was about 50° of Henley, sparks $\frac{4}{10}$ inch.

Third Storm.—At $1^{\text{h}} 40^{\text{m}}$ P.M. a third heavy shower consisting of rain and hail began, when Volta stood at $10^{\text{div.}}$ pos., but the charge immediately changed to neg., when the Henley vibrated between 60° and 90° , sparks $\frac{7}{10}$ inch. The positive maximum during this shower was about 60° of Henley. At 2 P.M. very stormy with heavy rain. At 3 P.M. dull and cloudy. At 4 P.M. heavy snow. From sunset to 10 P.M. dull and cloudy.

GREENWICH.

First Storm.—At the nearest observation, Jan. $30^{\text{d}} 22^{\text{h}}$ (Göttingen $9^{\text{h}} 20^{\text{m}}$ A.M. Greenwich time), the observer records: "A few clouds only here and there: at $8^{\text{h}} 5^{\text{m}}$ A.M. rain and sleet began falling; and about $8^{\text{h}} 40^{\text{m}}$ A.M. snow fell thickly, soon covering the ground; it ceased about $9^{\text{h}} 20^{\text{m}}$, when the clouds broke: wind in gusts to 2, with prolonged lulls." Negative electricity was observed between 7 and 9 A.M. very weak. Wind N.W., force $\frac{1}{4}$ to 7 lbs., rain falling occasionally.

Second Storm.—Jan. $31^{\text{d}} 0^{\text{h}}$, Göttingen $11^{\text{h}} 20^{\text{m}}$ A.M. Greenwich. Cirro-stratus and scud; wind in heavy gusts to $2\frac{1}{2}$ and 3. At $11^{\text{h}} 30^{\text{m}}$ A.M. sparks occurred from 0.05 inch to 0.13 inch in length, 2 in a second. Wind N.W., force 12 lbs. At this time a sudden squall of hail, wind and rain occurred; in an instant the gold leaf of the dry pile apparatus was destroyed, and in removing it the observer received a severe shock.

Third Storm.—At $1^{\text{h}} 20^{\text{m}}$ P.M. Greenwich time. Cirro-stratus; wind in heavy gusts; squalls of hail and snow are frequent; occasionally, also, a few breaks occur: very dark and gloomy; snow mingled with sleet has again begun to fall. Wind N.W., force 0 to 5 lbs. No electricity appears to have been observed.

Between 6 and 9 P.M. negative electricity was observed at Greenwich. Wind W.N.W., force 0 to 2 lbs. Sleet occasionally falling in small quantities: strong gusts of wind.

(^h) *February 9, 1844.*—Cirro-stratus was registered at Greenwich from $9^{\text{h}} 20^{\text{m}}$ P.M. to $5^{\text{h}} 20^{\text{m}}$ A.M. of the following morning; two snow-showers occurred during this period, one at 11 P.M., the other at $4^{\text{h}} 10^{\text{m}}$ A.M., the electrometer-bell ringing during their continuance. Electrical observations were made between the undermentioned times:

d	h	m	h	m	P.M.	max. tension	Volta	(2)	neg., sparks	in.
Feb. 9	10	55	to 11	35	P.M.	50	Volta	(2)	neg., sparks	0.10
"	9	11	40	" 11	54 P.M.	"	"	(1)	"	0.10
"	10	4	10	" 4	26 A.M.	"	"	(1)	"	0.10

(ⁱ) *February 26, 1844.*—We have in the case before us another instance (see Sept. 10, 1843) of the electrometers being affected by the approach of a cumulo-stratus, and on the present occasion *previous to the falling of rain*. It would appear from the ordinary meteorological observations at Greenwich that the few drops of rain recorded in the electrometer observations at $1^{\text{h}} 12^{\text{m}}$ P.M. were succeeded by a heavy squall of rain, which commenced at $1^{\text{h}} 15^{\text{m}}$ P.M. and continued 10 minutes; the negative charge continued until

1^h 55^m P.M. It is worthy of remark, that *the approach of the cloud to the zenith, the formation of the heavy rain-drops, and the affection of the instruments, the charge being negative, were apparently simultaneous*, and succeeded by the sudden gusts of rain constituting the heavy squall.

(^k) May 18, 1844.—The *contrast* between the observations at Kew and Greenwich is interesting: it furnishes us with another instance (and perhaps the most striking of the three) of the affections of the instruments by the proximity of cloud, most probably *cirro-stratus*, which was prevalent at Greenwich, at least before noon. During the changes that occurred there in the electrical charges, *small quantities of sleet only fell*, and these not in any degree measurable, for we find on May 18, 22 hours Göttingen time, the same records of the rain-gauges as on May 17, 22 hours; but at Kew the period marked by the affections of the instruments at Greenwich is characterized by three showers, two of which are recorded as *heavy*, the electrical changes being considerable. It is to be remarked, that at Greenwich the tension was *higher* than had been observed previously in the course of the year. These phænomena appear to point to a common origin of the electricity noticed at the two observatories, viz. the presence of a particular kind of cloud. It cannot in this instance at least be immediately connected with the rain, for although *the changes were manifested at Kew during the continuance of the showers, yet electricity of a greater tension than any that had been observed during the former part of the year was recorded at Greenwich; the same action was going on at Greenwich without the rain as at Kew with it*: the only difference appears to have been, the absence at Greenwich of those particular conditions necessary to the production of the sudden gush of rain most frequently characterizing the exhibition of negative electricity, or rather the oscillation of the electrical condition between positive and negative. The instance before us presents a very instructive comparison with the passage of the cloud over the Kew Observatory on September 10, 1843, when the conditions for the production of rain did not appear to have existed at Kew, while they did at Greenwich; yet the electrical instruments at Kew *were affected*, while those at Greenwich *were not*.

(^l) June 10, 1844.—The records of this shower at both observatories were as under:—

Kew.

Previous to the fall of any rain upon the conductor, the Henley rose to 90° pos., sparks $1\frac{1}{2}\frac{9}{10}$ inch*. At one time of this high positive charge (before the rain), the Leyden jar, of about 56 square inches coating, on being applied to the conductor, became charged to the intensity of the rod in about 20 seconds. The charge changed to negative shortly after the rain began, max. 55° of the Henley, sparks $\frac{2}{5}$ inch. These high signs lasted about a quarter of an hour, and spiritings occurred from the little ball above the discharger. The negative charge remained a considerable time after the rain had ceased, gradually diminishing.

Nothing remarkable in the appearance of the clouds; they were rather fleecy or plumose, and not low, but large.

* These were the *longest sparks* which we have yet observed; but on the 31st of January the *continuous stream* of fire from the conductor to the discharger was much more lasting. If the ball attached to the conductor and above the discharger were placed nearer or at the end of the cross-arm, the sparks would be longer; also if it were smaller. But it is, I fear, in vain to attempt to measure these very high tensions accurately by ordinary electrometers and dischargers. Our Henley was in this instance evidently useless. The shock of the spark reached the elbow without a jar. [Observer at Kew.]

GREENWICH.

June 10^d 2^h Göttingen time, 1^h 20^m Greenwich time. Cumuli, cumulostrati, and dark scud; within the last three minutes the temperature has fallen 3°, the reading just before the observation having been 74°·5, and there was a sudden exhibition of negative electricity; a large dark cloud was at the time passing over from the N.W.: at 1^h 27^m P.M. a fine shower of rain began falling; at 1^h 29^m the temperature was 62°·0; and at 1^h 46^m it was 59°·5.

Negative electricity recorded between 1^h 16^m and 1^h 44^m P.M., max. tension 20° Volta (2). Wind W., force 0 to 1 lb., rain falling.

By means of these records we obtain a further insight into the conditions necessary for the exhibition of the phenomena detailed. Cloud being the origin of the electrical oscillations, appears very evident from the affections of the instruments at Kew *previous to the fall of any rain upon the conductor*; and the very *high* charge communicated to the conductor under these circumstances is highly instructive. The usual march of the electrical tension was evidently disturbed by the approach of the cloud, although it exhibited *nothing remarkable*. This disturbance did not manifest itself at Greenwich until the cessation of the rain at Kew. It appears that at this time the observer at Greenwich noticed *a large dark cloud passing over from the north-west*, which was attended by two very remarkable phenomena:—*a sudden diminution of temperature, with as sudden an exhibition of negative electricity*. This appears to have occurred at least *seven* minutes before the fall of any rain. The presence of the cloud, the diminution of temperature, and the exhibition of negative electricity, appear to be closely and intimately connected, and to indicate either that the cloud itself underwent a remarkable physical change, which materially influenced bodies in its vicinity; or, which is the most probable, that it existed in such a condition as to produce great physical changes in such bodies, so far as electricity is concerned. It is easy to conceive, that if by any means the temperature of the cloud should be diminished; by coming into a colder portion of air, for instance, a sudden agglomeration of its vapour-particles might take place; its electrical condition be suddenly and extensively disturbed by the enormous tension which these newly formed rain-drops might acquire in consequence of the rapidity of their formation, in some cases the diminution of temperature being so great as actually to freeze them and thus produce hail, which at some seasons is not an unfrequent phenomenon accompanying the exhibitions of negative electricity. The electrical influence of the cloud thus circumstanced may not be confined to the mere strip of country over which the rain or hail may fall, but may extend to some little distance beyond its circumference, and thus the signs may be changed without the actual fall of rain in such localities, or the negative state continue after the precipitating portion has left the place of observation. Nor does it follow that rain must necessarily fall from *every* portion of the under-surface of a cloud; there may be an axis characterized by the *lowest* temperature; around this may exist a zone having a *higher* temperature, and another *still higher*, the skirts exhibiting the *highest*.

It is well known that in showery weather the masses of cumulus present the appearance of *highly heaped or vastly piled-up clouds* towering high in the atmosphere, and on many occasions these cumular bodies are surmounted by sheets of cirro-stratus, through which their summits frequently penetrate, giving rise to that modification of cloud termed by meteorologists cumulostratus. By carefully noticing their mode of formation the idea will be suggested of vapour rising from the earth by evaporation *with considerable force*,

and which upon passing the vapour-plane is immediately condensed. The supply continuing from below, and the condensation going on above, produce the heaping, piling-up, and general outline of the cloud—which is particularly characterized by its *crenated* edges, and to which it owes its picturesque appearance—just as steam, which, issuing in an invisible state from the funnel of a locomotive, meets with a stratum of air sufficiently cold to condense it *rapidly*, by which it assumes in a very decided manner the characters of the highly-heaped cumular clouds. It has been suggested, that the immense masses of these clouds, so commonly met with in the calm latitudes between the trades, may possess some such an arrangement as above-mentioned—at least in the temperature of the rain that falls from them—by their more *elevated* portions being precipitated by the colder air with which they come in contact; and as it is likely the most elevated part of the cloud would most probably be situated near its centre, the precipitated rain would fall along the axis, and bring with it to a greater or less extent the temperature which contributed to its formation. The other portions of the cloud not being so elevated as the central would produce rain of a higher temperature, the rain falling from the skirts of the cloud being the warmest.

One such cloud appears to have come under the writer's notice, at least if the *difference* in the *precipitations* may be regarded as indicating differences of *temperature*, or of *elevation* of certain portions of the cloud. The cloud was considered to extend over a diameter of about six miles; near the *axis* a fall of *snow* occurred which was *surrounded* by a precipitation of *hail*, and from the portions near the *skirts*, *rain* fell. It would appear that the temperature in the centre or axis was sufficiently low, or that the summit of the cloud was sufficiently elevated to *freeze the vapour-particles before they had run into drops* in the usual manner in which snow is formed; but in the zone characterized by the *fall of hail*, a different process appears to have contributed to its production. Upon the first formation of the drops, the temperature appears to have been *above* the freezing-point, and it is possible that the relative diminution of temperature in this zone might have been greater than in either the axis or skirts. If so, we have all the conditions for a very rapid formation of rain-drops, which, from their proximity to the snow on the one hand, and the continued diminution of temperature on the other, might soon become *frozen*. There can be no question but that so rapid a conversion of aqueous vapour from the *aëriform* to the solid state, must have been accompanied by electrical *phænomena* more or less striking; the electrical condition of the cloud itself, as before observed, must have been materially influenced, and this as it travelled onwards again influenced bodies in its more immediate neighbourhood as it passed them. In the observations more immediately before us, as well as in numerous others, we find that shortly after the *rain began* the charge became *negative*. That the *cloud* disturbed the usual electrical condition of the conductor is very evident from the observations, and it is to be presumed that, at the time the high positive charge was communicated to the conductor, *the heavy rain was falling, although it had not arrived at the observatory*;—in other words, that portion of the cloud in which the diminution of temperature was so great as to occasion the rapid formation of rain, and thus alter the electrical condition of the cloud itself, was yet at some distance from the observatory. There might possibly have been at this moment *two* bodies reciprocally acting on each other electrically—the body of falling rain and the cloud; and it may not be at all improbable that it is the actions of these bodies, the one on the other, that influence our conductors, and give rise to the sudden and extensive changes often recorded on the occurrence of squalls of rain, hail and snow. The di-

minution of temperature in the present instance at Greenwich was 15° in 26 minutes, but nothing further than the fall of a *fine shower of rain* occurred; probably the path of the *heavy rain* did not cross the Greenwich Observatory, although the instruments there were influenced.

(*m*) *June 18, 1844.*—This thunder-storm, which exhibited very interesting phenomena at Kew, did not extend eastward so far as Greenwich; neither thunder, lightning, rain, nor any affections of the electrical instruments were observed there; the only record at all bearing on the subject is one that indicates the presence of *cirro-stratus*. During the whole time the sky was completely overcast at Greenwich. As illustrating the rapid succession of phenomena on these occasions, as well as some of the suggestions in the preceding note, it may not be uninteresting to subjoin the entire record of the observations at Kew.

TABLE XCIV.

Phænomena of a Thunder-storm observed at Kew on June 18th, 1844.

Time.	Phænomena.	Tension.	Spark.	Wind.
h m			in.	
3 40 p.m.*	Rain beginning	Henley 22 P.	S.
3 45 p.m.	Henley 40 P.	0·300	S.
3 46 p.m.	Henley 50 P.	0·400	S.
3 55 p.m.	Distant thunder	Henley 25 P.	S.S.E.
4 0 p.m.	Distant thunder	Henley 5 N.	S.S.E.
4 2 p.m.	A flash	Henley 60 N. (<i>a</i>)....	S.S.E.
4 4 p.m.	Distant thunder; no rain	Henley 65 P. (<i>b</i>)....	S.
4 8 p.m.	Distant thunder; no rain	Henley 60 N.	S.
4 10 p.m.	A few drops of rain	Henley 59 N.	S.
4 13 p.m.	A flash †	Henley (<i>c</i>)	
4 15 p.m.	A flash ‡	Henley 60 N. (<i>d</i>)....	
4 21 p.m.	Distant thunder; a little rain	Charge gradually falling.	
4 24 p.m.	A flash	No effect on electro meter.	S.S.W.
4 27 p.m.	Rain increasing	Volta 10 P.	S.
4 34 p.m.	Heavy rain	Henley 35 P. (<i>e</i>)....	0·300	S.
4 35 p.m.	Heavy rain	Henley 40 P.	0·350	
4 47 p.m.	Sudden fall and gradual rise of elec	trometer.	
4 51 p.m.	Heavy rain	Volta 7 N.	S.S.W.
5 0 p.m.	Heavy rain	Henley 5 N.	S.S.W.
5 4 p.m.	Heavy rain and distant thunder ...	Henley 20 N. (<i>f</i>)....	
5 15 p.m.	No rain	Henley 17 N.	S.E.
5 30 p.m.	No rain	Henley 15 N.	S.E.
5 37 p.m.	No rain	Volta 90 N.	S.E.
5 50 p.m.	No rain	Volta 6 P.	S.E.

The following notes by the writer of this report may probably assist in more distinctly particularizing the principal features of the above-recorded phænomena. The references are in letters of the italic alphabet.

(*a*) The occurrence of the flash and the increase of the negative tension may indicate the approach of the cloud as well as the formation of rain. It would appear that from 3^h 40^m to this time, 22 minutes, rain had been falling, but not such as to lead the observer to record it as heavy.

(*b*) The maximum tension; rain had ceased, but great oscillation of the charges existed.

(*c*) This flash appeared to exert a momentary influence on the conductor; the tension was slightly declining, but increased after its occurrence.

* At 3^h 35^m P.M. distant thunder and lightning, Volta at 50° pos. Distant thunder was heard at 3 P.M.

† Henley fell from 55° to 20°, and quickly rose again.

‡ No effect on the electrometer.

(d) This flash *appeared* to have no effect on the electrometer.

(e) The “gush of rain” *arrived* at the observatory. It may be remarked that after this, thunder was heard but once, and in all the records it is described as *distant*. From the time thunder was last heard, 4^h 21^m P.M., the charge had gradually fallen to Volta 10 div. P. The highest tensions were observed, not when the rain was heaviest, but when the discharges (at a distance) took place more frequently. It is probable that after the cessation of these discharges the “gush of rain” came travelling on, being still accompanied by the causes of its production, and a corresponding oscillation of the tension occurred.

(f) The increase of tension on the occurrence of the discharge is very apparent, as well as the *gradual* decline afterwards, notwithstanding the cessation of rain which occurred within the next 11 minutes.

(n) *July 1, 1844.*—As the records of this storm have already appeared in the volume of Reports for 1844, page 134, we shall not further introduce them to the reader. On a careful consideration of the record it will be found that the storm may very naturally be divided into three sections, viz. the period of heavy rain *previous* to the electrical discharges; the period of the discharges themselves; and the period of rain *succeeding* the discharges, a portion of which was *heavy*. The times are as follows:—first period 5^h 30^m P.M. to 5^h 55^m P.M. inclusive=25 minutes; second period 5^h 56^m P.M. to 6^h 24^m P.M. inclusive=29 minutes; third period 6^h 25^m P.M. to 7^h 50^m P.M. inclusive=1 hour 26 minutes. We have in the first period a decided instance of heavy rain, characterized on one occasion as *very* heavy, being in advance of the actual thunder-storm. During the second period neither *thunder* nor *heavy rain*, except on one occasion, appear to have been noted: it is however to be presumed, as we shall have occasion hereafter to notice, that from the frequency and character of the flashes they were accompanied by both, and the probability is, that during the exhibition of the lightning the rain that fell was much heavier than that in either the preceding or succeeding period. In the third period the heavy rain continued about half an hour. The values of the tensions having reference to these periods are interesting. The mean of the tensions recorded during the first, without having regard to kind, is 32° of Henley; that of the second 48° of Henley; and that of the third 27° of Henley, or during the heavy rain only, 33° of Henley. The connexion between the high tensions and the electrical discharges from the cloud is very apparent; also the mean values of the tensions during each period of the heavy rain indicate a certain relation between them. The entire phenomena strongly suggest the existence of an axis characterized by the active development of strong electric action; the tension of the cloud and probably that of the rain being so enormous that frequent discharges took place to restore the equilibrium. This axis occupied about half an hour in passing the observatory. It is probable the strong action going on in the centre was communicated to a zone of nearly the same breadth in all its parts, in which the principal phenomenon was *the rapid formation of rain* unaccompanied by electric discharges. In connexion with this it may be remarked that the third period may be subdivided into two, the first characterized by heavy and the last by light rain; the duration of the first was, as we have already noticed, 30 minutes, namely from 6^h 25^m P.M. to 6^h 55^m P.M. inclusive, and this may probably be regarded as the true termination of the storm. The three periods,—viz. preceding heavy rain; actual thunder-storm; and succeeding heavy rain—do not differ very considerably in duration from each other. The first = 25 minutes, the second = 29 minutes, and the third = 30

minutes. It is also to be remarked, that at the commencement and termination of the second, oscillations in the *kind* of tension occurred, the tension at the occurrence of the first flash being positive 60° of Henley, and that at the last also positive 50° of Henley: the intermediate tensions were negative. Oscillations also occurred during the periods of heavy rain.

At Greenwich the same storm was observed, the clouds recorded being cirro-stratus and scud. It appears to have commenced at $5^h 49^m$ P.M., at least so far as the affection of the instruments is concerned; the record is as follows:—[“This storm first rose in the N.W.; it then passed round to the north, and afterwards to the east, as also did the wind; at $5^h 50^m$ there was a vivid flash of lightning, followed by thunder at the interval of seven seconds; at $5^h 55^m$ there was another very bright flash, and thunder followed at an interval of two seconds; this was a long peal, the crackling continuing from 45^s to 59^s . Several flashes of lightning took place between 6^h and $6^h 15^m$, followed by thunder at intervals of one, two and three seconds. Between 6^h and $6^h 20^m$, 0.78 inch of rain fell at Mr. Glaisher’s residence; after this time the lightning ceased; the rain however continued, but not so heavily.”—G.]

From this record it may be gathered that the first flash of lightning occurred at $5^h 50^m$ P.M., being six minutes earlier than the occurrence of the first flash at Kew; it is described as very vivid, and followed by thunder at the interval of seven seconds. The second flash, which was very bright, occurred at $5^h 55^m$ P.M., one minute earlier than the first at Kew; it was evidently much nearer than the first observed at Greenwich, the interval being two seconds. Between 6^h and $6^h 15^m$ P.M. several flashes are recorded, the point of discharge being upon the whole nearest to the observatory during this quarter of an hour. During the same period six flashes were registered at Kew, from four of which sparks were obtained, the longest being 0.4 inch; it occurred at $6^h 5^m$ P.M. This quarter of an hour was evidently the period in which the focus of the storm passed *both* observatories, and during the twenty minutes between 6^h and $6^h 20^m$ Mr. Glaisher registered 0.78 inch of rain at Blackheath. It is this circumstance to which we wish to refer in connexion with the *axis* of the storm, it being evidently accompanied at Blackheath by a great precipitation of rain. Less rain appears to have fallen at Greenwich, about half an inch having been registered during the twenty-four hours from $9^h 20^m$ A.M. of July 1 to $9^h 20^m$ A.M. of July 2. During the storm changes of tension occurred, the maximum tension being 30° of Henley and the longest spark 0.23 inch.

(*o*) *July 5, 1844.*—Between $11^h 18^m$ A.M. and $1^h 15^m$ P.M. a thunder-shower passed over the observatory at Greenwich. Positive and negative electricity were exhibited; heavy cumulo-strati covered the sky until $11^h 55^m$ A.M., when heavy rain began to fall and thunder was heard in the N.W.; max. tension 10° of Henley; sparks max. length 0.13 inch. During this time the weather at Kew is registered “fine but cloudy,” but at 1^h to $1^h 5^m$ P.M. a heavy shower of rain is recorded, which does not appear materially to have affected the instruments.

Between $4^h 0^m$ P.M. and $4^h 46^m$ P.M. changes are again recorded at Greenwich with rain falling; the electricity was negative until $4^h 12^m$ P.M., when it suddenly became positive, max. tension observed 120 div. Volta (2). During the whole of this time the charge was negative at Kew.

(*p*) *August 8, 1844.*—There can be but little doubt that the fine rain at a distance observed at Kew at $1^h 26^m$ P.M. is the same shower that fell at Greenwich at $1^h 35^m$ P.M.; the only link in the chain of evidence required to identify it is the *direction* in which the fine rain was seen from Kew; both

conductors were affected almost simultaneously. If the shower *seen* at Kew and the one that *fell* at Greenwich be the same, we have another instance of the cloud being the common origin of the electricity exhibited at the two observatories*.

It has already been remarked, that one of the most prominent results of the arrangement constituting Table XCII. is the almost constant accompaniment of *rain in a falling state* when the conductor exhibits a negative charge, and it is to be particularly noticed that this is in striking contrast with the condition of the atmosphere surrounding the conductor when *high charges of positive electricity are exhibited, the tension not being in a state of oscillation*. In both cases the conductor may be said to be surrounded by moisture, but the conditions of this moisture are extremely different. In the case of high positive tension such as we have described, the moisture *is not in the liquid state*; and even if it may be said to be in contact with the surface of the conductor, yet it has not passed beyond the form in which it exists as cloud; *the conductor under such circumstances may be considered as penetrating the cloud; and bringing to us the electricity of the cloud itself*. In the case of falling rain, the conductor is situated *below* the cloud, the drops impinge on it, and it is evidently a matter of question whether its indications are those of the electricity of the rain, or of a state induced in the conductor by the proximity of the cloud. A note appended to the description of instruments at Kew (Report 1844, page 124), relative to Henley's electrometer, appears to lead to the conclusion that the latter is the case:—"The oscillations of the index between the 30th and 35th degrees, sometimes during a heavy shower, plainly show that the electricity of the conductor is washed off, as it were, as fast as brought." By the electricity of the conductor being *washed off*, as it were, it would appear that the electric state induced in the conductor was momentarily conveyed *from it* by the falling rain. In connexion with this, we must bear in mind that *all* rain is not accompanied by negative electricity, nor on the other hand is the negative charge *always* accompanied by rain. In those instances in which negative electricity has been observed without rain, the state of the weather is printed in italics in Table XCII., and in such cases the presence of cloud *alone* has been the accompanying phenomenon at the Kew Observatory; nevertheless on some of these occasions heavy rain has fallen at Greenwich. If therefore negative electricity should be, as it appears to be, connected with cloudiness, it ought to present a

* It is a remarkable circumstance and one demanding further attention, that most of the *thunder-storms* recorded in the foregoing pages passed more or less to the *north-west* of the Royal Observatory at Greenwich. We give the following as illustrative of this remark:—

August 4, 1843..... N. and N.W.
June 10, 1844. N.W.
July 5, 1844..... N.W.

August 15, 1843. N.E., S.E., N.W.
July 1, 1844..... N.W., N., E.

To these instances we may add that of the remarkable thunder-storm which passed over London on July 26, 1849. In the meteorological observations furnished by the Astronomer Royal, and published in the weekly report of the Registrar-General, it is thus noticed:—"From 1^h till 4^h P.M. a violent thunder-storm, *chiefly situated to the north*; the flashes of lightning were vivid and in quick succession, followed by loud thunder at intervals of 15 to 20 seconds generally." The storm passed over London from S.W. to N.E., striking several buildings in its passage. During the continuance of the storm at Greenwich the electrical tension was strongly *positive* for a period of two hours and a half, viz. from 1^h to 3^h 30^m *while the storm raged in London*; at other times, the observer writes, the tension was strongly negative, with frequent constant volleys of sparks and galvanic currents.

From the above it may be inferred that London is more particularly exposed to the effects of thunder-storms, most of them passing over the immediate neighbourhood of the metropolis.

diurnal period more or less in harmony with it. We have already remarked, that the record of negative exhibitions does not furnish us with sufficient data previous to 1845 to determine the diurnal period; nevertheless a synoptical arrangement of the hours included in the entries under the head "Limits of Time," furnishes us with an approximation to such a period—at least so far as the *time* of occurrence of negative charges is concerned. The following table, which is deduced immediately from Table XCII., exhibits the number of times negative charges (more or less) were observed between August 1843 and December 1844, both inclusive, between the hours specified, making in the whole 231.

TABLE XCV.

Number of readings of negative electricity between the hours specified,
from August 1843 to December 1844.

		Between																															
1	5 & 6 a.m.	2	7 & 8 a.m.	3	9 & 10 a.m.	4	10 & 11 a.m.	5	11 & Noon.	6	Noon & 1 p.m.	7	1 & 2 p.m.	8	2 & 3 p.m.	9	3 & 4 p.m.	10	4 & 5 p.m.	11	5 & 6 p.m.	12	6 & 7 p.m.	13	7 & 8 p.m.	14	8 & 9 p.m.	15	9 & 10 p.m.	16	10 & 11 p.m.	17	Sum.

It appears from this table that during the seventeen months negative electricity was not observed earlier than 5 A.M.: at the commencement of the series the numbers are small, but they increase gradually until 11 A.M., immediately after which hour they are doubled as compared with the preceding three hours. This value slightly decreases until between 2 and 3 P.M., and is again augmented between 3 and 4 P.M. A sudden diminution occurs between 5 and 6 P.M. The numbers from 5 P.M. to 8 P.M. are rather higher than those from 8 A.M. to 11 A.M., and late in the evening they are again few as at the commencement. The period of the day between 11 A.M. and 5 P.M. is particularly characterized by the more frequent exhibition of negative electricity than either the forenoon or evening, and the ratio as compared with these periods is very considerable. It is remarkable that so close a correspondence as regards the development of negative electricity in the middle of the day should obtain in the series of negative readings previous to 1845 and during the three succeeding years (see Table III. page 117). It is perfectly clear that the greatest number of negative readings occurs about the middle of the day, and this of itself would suggest the great probability of the existence of a diurnal period in the exhibition of negative electricity.

TABLE XCVI.

Mean amount of cloud at each observation-hour, Göttingen mean time, as deduced from the observations of six years at the Royal Observatory, Greenwich, and expressed in parts of the natural scale,—a sky completely covered with clouds being represented by 100.

Mid.	2 a.m.	4 a.m.	6 a.m.	8 a.m.	10 a.m.	Noon.	2 p.m.	4 p.m.	6 p.m.	8 p.m.	10 p.m.	Mean.
61	65	67	69	70	71	71	71	69	66	62	60	67

TABLE XCVII.

Comparison of the negative readings at Kew previous to 1845, with those also at Kew from January 1845 to July 1848 inclusive, and both with the mean amount of cloud at Greenwich from 1841 to 1846 inclusive, at hourly and two-hourly intervals.

A.M.													P.M.												
	M.	1	2	3	4	5	6	7	8	9	10	11	N.	1	2	3	4	5	6	7	8	9	10	11	
Neg....	1	5	9	11	10	11	22	21	21	17	23	22	14	12	13	10	7	2	...	
Neg....	8	...	12	...	12	...	18	...	34	...	56	...	46	...	52	...	55	...	60	...	38	...	33	...	
Cloud	65	...	67	...	69	...	70	...	71	...	71	...	71	...	69	...	56	...	62	...	60	...	61	

The numbers in these tables agree, so far as the general fact is concerned, in exhibiting a greater quantity of negative readings during a portion of the day which is distinguished by a greater prevalence of cloud. Dividing the day into two periods, viz. from 8 A.M. to 8 P.M. and from 8 P.M. to 8 A.M., we find that the *occurrence* of negative electricity is very considerable in the day as compared with the night. In the three years 1845 to 1847 (including also the first seven months of 1848), which furnish a comparable scale of numbers with regard to the cloudiness, the proportion of night to day negative readings is as 2 to 5 very nearly. The same portion of the day, viz. from 8 A.M. to 8 P.M., gives, as compared with the remaining twelve hours, the greatest prevalence of cloud, the mean amount being about 68 hundredths of the whole sky: during the night the mean amount is 65 hundredths, or about three hundredths less. In connexion with this, it may be remarked that the greater prevalence of cloud is rather in advance of the development of negative electricity: the period from 7 A.M. to 7 P.M., and *vice versa*, gives double the difference between the day and night cloudiness; the mean amount in this case for the day being very nearly 7 tenths, while that for the night is 64 hundredths, or about 6 hundredths less. The proportion of the negative readings is the same. From Table XCII. it may be inferred that on most occasions when negative electricity occurred, *the sky was entirely covered with clouds*; and this might suggest that it is not so much the general existence of cloudiness in the atmosphere that may be connected with negative electricity, as the presence of certain clouds—*cumulo-stratus* for instance, or more probably *cirro-stratus*, from its almost constant occurrence with negative electricity. The remarkable changes that frequently occur from one kind of electricity to the other, often very suddenly, and at the same time very considerable in intensity, clearly show that at the time disturbances of no ordinary character prevail, and it may readily be conceived (in addition to the suggestion already offered) that different strata of cloud in different electrical states, operating on each other and on the earth, may very violently disturb the ordinary march either of the electricity of serene weather or of the aqueous vapour; and although these disturbances (taking them singly and considering the great uncertainty of their occurrence) may be regarded as purely accidental and obeying no recognized law of periodicity, yet should they result from causes which in themselves are not subject to mere accidental manifestations, but are the results of forces operating on the earth's atmosphere in a definite manner—producing for instance a greater accumulation of cloud at one period of the day rather than at another, and giving rise to a well-defined march in the manifestation of the cloudiness of the atmosphere, within small limits it is true, but yet sufficient, from six years' careful observation, to characterize the curve as that of a single progression

having an ascending and descending branch, the maximum occurring about 40 minutes before noon, and the minimum between 9 and 10 at night—then they must necessarily exhibit somewhat of the same subjection to the laws of periodicity which is characteristic of the causes themselves. That the *diurnal occurrence* of negative electricity is of a periodical character, the observations of five years, viz. from August 1843 to August 1848, testify in a very unequivocal manner; and although its connexion with the general cloudiness of the atmosphere may not be satisfactorily made out, yet it by no means follows that it may not be more immediately connected with certain classes of cloud; for as we have determined a diurnal period in the cloudiness generally, it is not unlikely that certain clouds, the *cirro-stratus* for instance, may likewise exhibit a diurnal period, being much more frequent in its occurrence at one portion of the day rather than at another. Upon the whole, the negative readings are obvious indications of considerable disturbances, and their occurrence in much greater frequency at a particular period of the day renders it highly probable that the disturbances themselves are of a systematic character and subject to well-defined laws of diurnal periodicity.

Negative readings from January 1845 to July 1848 inclusive.—During this period 424 negative charges of the conductor were observed. Their distribution among the twelve observation-hours is seen in the following table, which also includes the mean value of the negative tension at each observation-hour, and the excess or defect of such mean as compared with the mean of the whole.

TABLE XCVIII.

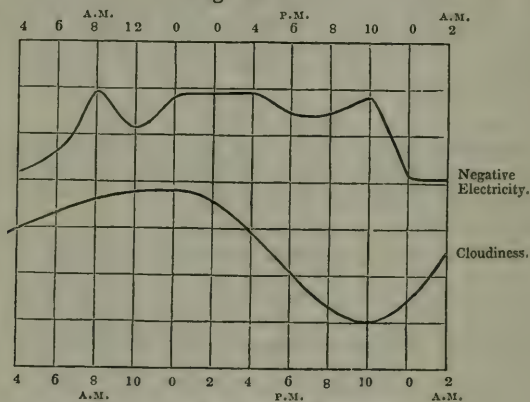
Number of readings, mean tension, and excess or defect above or below the mean of all the negative observations from January 1845 to July 1848, as referred to the twelve observation-hours.

	Mid.	2 a.m.	4 a.m.	6 a.m.	8 a.m.	10 a.m.	Noon.	2 p.m.	4 p.m.	6 p.m.	8 p.m.	10 p.m.	Sums and Means.
Readings	8	12	12	18	34	56	46	52	55	60	38	33	424
Tensions.	div.	div.	div.	div.	div.	div.	div.	div.	div.	div.	div.	div.	div.
Excess	36·0	24·8	109·4	316·3	938·6	566·2	871·7	891·3	907·6	729·9	721·9	870·2	725·3
or	—	—	—	—	+	—	+	+	+	+	—	+	—
Defect.	689·3	700·5	615·9	409·0	213·3	159·1	146·4	166·0	182·3	4·6	3·4	144·9	725·3

We have already alluded to the greater frequency of the occurrence of negative electricity in the middle of the day, and have remarked that the period under consideration agrees with the previous seventeen months in this particular. The line of mean tensions in the above table, in addition to the greater frequency of occurrence in the middle of the day, exhibits upon the whole period a corresponding increase of tension, particularly from 8 A.M. to 4 P.M., a portion of the day characterized by the greater prevalence of cloud (see Table XCVI.). The maximum occurs at 8 A.M., but from the close approximation in the values of the mean tensions at noon, 2 and 4 P.M., it can hardly be considered as the true maximum of the diurnal period: it is to be remarked that only 34 observations contribute to its determination, and until a more extended series can be obtained, it must remain a matter of question. The mean tensions at noon, 2 and 4 P.M., taken in connexion with those at 10 A.M. and 6 P.M., present a well-rounded and very regular portion of a curve, which in the absence of further observations may probably be considered as representing at least approximately the portion of the diurnal period of negative

electricity from 10 A.M. to 6 P.M. At 8 P.M. the diminution is so exceedingly slight as almost to indicate a tendency to rise at that hour, and at 10 P.M. we have a decided increase: but in connexion with this, it should be borne in mind, that at one of the 33 observations contributing to its determination, the Henley's electrometer read 70° ; and it is easily seen that this high tension very materially influences the result, for if we abstract it, the mean tension is lower than that at 8 P.M. With regard to the mean tensions at midnight and 2 A.M., the same remarks apply which we offered relative to the positive tensions at these hours (see pages 118, 119); they are for the same reason probably lower than the truth, and indeed more particularly so in the case of negative electricity; for it is likely that when such electricity has been indicated by the conductor on other occasions than the eight and twelve recorded, it has exhibited much higher tensions than 50 div. of Volta No. 1. The remarkable difference between the values of the mean of all the positive observations for three years (66.9 div.) and of all the negative during 43 months (725.3 div.) is exceedingly interesting, as indicating at once the character of the movements giving rise to the negative exhibitions, viz. *disturbances*.

Fig. 19.



Diurnal Curves of Negative Electricity and Cloudiness.

The annexed curves (fig. 19) exhibit to the eye the principal *diurnal* phenomena of negative electricity and cloudiness: 1000 divisions of Volta's electrometer No. 1 are considered equal to two vertical divisions of the scale on which the negative tensions are projected; eight hundredths of the scale of cloudiness being also considered equal to two of the same divisions. The points of the curve of cloudiness are placed about one-third of each horizontal division from the vertical or hour lines, the determination being at even hours of Göttingen mean time. The greater prevalence of cloud being in advance of the exhibition of negative electricity, which we noticed when treating of the frequency of its occurrence in the middle of the day, is very striking in the curves before us, which show that the same phenomena obtain in the comparison of the two, whether we regard the *occurrence* or the *value of the tension* of negative electricity. There is also another feature which ought not by any means to be overlooked; it is the similarity in this respect that exists between the curves of negative electricity and cloudiness, and those of the annual period of positive electricity and humidity (see page 153). In

both instances the cloudiness and humidity precede the electricity, and strongly indicate that whatever relation may exist between the development of positive electricity and humidity on the one hand, and that of negative electricity and cloudiness on the other, such relations are not only likely to be of a very constant character, but that a similarity exists between the two sets of phenomena which goes far to show that the nature of their connexion, if any, is also similar; the one, viz. positive, principally indicating, as we have before remarked, the electric tension of aqueous vapour; the other, viz. negative, the electrical disturbances produced by the sudden precipitation of this vapour when existing as cloud.

It would greatly contribute to our knowledge of this part of our inquiry, if systematic and comparative observations were instituted at different observatories, on occasions of electrical disturbances, of a somewhat similar character, but of course considerably varied in their details, to those adopted on the occasions of magnetic disturbances. A principal feature in such observations should be *the observation of the electrometers at regular but small intervals of time during the continuance of the disturbance*, so that curves of the variations of the instruments might be readily projected at any time afterwards. Provision should also be made for noting the *precise instants at which particular and striking phenomena occurred*, such as *lightning, thunder, a change in the kind of electricity, the commencement of rain, the commencement of heavy rain, the termination of rain either light or heavy, also the same phenomena as regards hail or snow*. A rain-gauge should also be *kept for these particular phenomena*; it should be of such a construction as to admit of its being frequently read during the continuance of the disturbance; and its indications should be noted at sufficiently short intervals to afford data from which a curve could be constructed by which the eye could readily judge of the lightness or heaviness of the rain by the *amount precipitated* within the interval fixed on. Observations of the kind just alluded to should by no means be confined to the more striking exhibition of electrical phenomena, such as thunder-storms, &c., but upon the slightest indication of a disturbance they should be immediately resorted to; even on the positive tensions ranging higher than usual, the shorter intervals of observation may with great propriety be adopted, if it should be only for the purpose of securing on such extraordinary occasions the epoch of maximum; and in all instances that it may be deemed advisable to resort to them, they should be continued while there is the least indication, either from the appearance of the sky or from the instruments, of the existence of the disturbance, and in fact until the observer is perfectly satisfied that it has ceased. It may be well to remark, that electrical disturbances appear to be very *confined* in their effects, extending over but a comparatively small portion of the earth's surface.

MR. MALLET'S Report On the Facts of Earthquakes does not appear, as intended, in the present Volume, in consequence of the manuscript having been delayed by the author, pending his researches in foreign libraries, until too late for the period fixed for publication.

The Report will appear in the Volume for next year.

NOTICES
AND
ABSTRACTS OF COMMUNICATIONS
TO THE
BRITISH ASSOCIATION
FOR THE
ADVANCEMENT OF SCIENCE,
AT THE
BIRMINGHAM MEETING, SEPTEMBER 1849.

ADVERTISEMENT.

THE EDITORS of the following Notices consider themselves responsible only for the fidelity with which the views of the Authors are abstracted.

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NOTICES AND ABSTRACTS

OF

MISCELLANEOUS COMMUNICATIONS TO THE SECTIONS.

MATHEMATICS AND PHYSICS.

On the Application of Graphical Methods to the Solution of certain Astronomical Problems, and in particular to the Determination of the Perturbations of Planets and Comets. By J. C. ADAMS, F.R.S.

AFTER briefly pointing out the advantages of graphical methods, the author proceeded to give some instances of their practical application. It was shown that the solution of the transcendental equation which expresses the relation between the mean and excentric anomalies in an elliptic orbit, is obtained in the most simple manner by the intersection of a straight line with the curve of sines. Attention was directed to Mr. Waterston's graphical method of finding the distance of a comet from the earth, and an analogous method was given for determining the distance of a planet, on the supposition that the orbit is a circle in the plane of the ecliptic.

The author then passed on to the more immediate object of his communication, the graphical treatment of the problem of perturbations of planets and comets. He first showed how to obtain geometrical representations of the disturbing forces, and then gave simple constructions for determining the changes produced by these forces in each of the elements of the orbit, in a given small interval of time. Having obtained the total changes of the elements in any number of such intervals, it was shown in the last place how to find their effect on the longitude, radius vector and latitude of the disturbed body, and thus to effect the complete solution of the problem of perturbations without calculation.

On a Model of the Moon's Surface. By HENRY BLUNT.

This model is an accurate representation of a part of the moon's surface as it appears through a Newtonian telescope of seven feet focus and nine inch aperture, under a magnifying power of about 250. The large volcanic crater, which forms the principal object in the model, has received the name of Eratosthenes. It is about thirty miles in diameter and stands at the end of a lofty range of mountains not far from the centre of the moon's disc. A hilly district, rising into two or three lofty peaks, runs upwards from Eratosthenes, connecting it with what appears to have been an ancient crater now filled up. Touching the edge of this crater and descending from it towards the right, may be seen a long line of minute volcanic cups, which are nearly the smallest objects visible with the instrument by which the observations were made. The whole is represented as seen with an inverting eye-piece, and the model ought to be held in an oblique light in order to view it to advantage.

On some new Applications of Quaternions to Geometry.
By Sir W. HAMILTON.

On the Heat of Vaporization of Water. By J. P. JOULE.

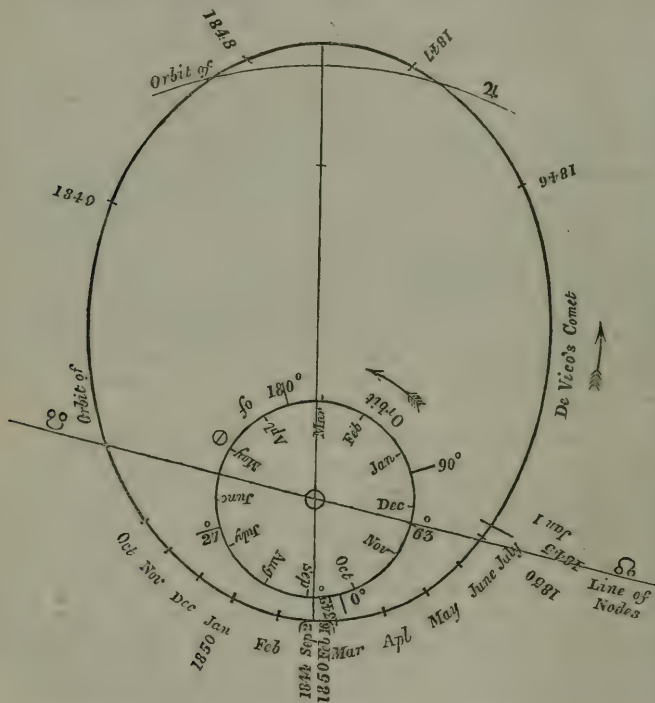
The object was to point out the complex nature of the heat hitherto taken for the latent heat of steam. In the exact experiments of Regnault 965° was found to be the quantity of heat evolved in the condensation of steam saturated at 212°; of this quantity 75° was stated by the author to be the heat due to the *vis viva* com-
1849.

municated by the pressure of the steam, leaving 890° as the true heat of vaporization of water. In a perfect steam-engine, supplied with water at 212° and worked at atmospheric pressure without expansion, 965° will be the heat communicated from the fire to the boiler, 75° will be the heat utilized by conversion into force, and the remainder, 890° , will be the heat given out in the condenser.

By working the steam expansively, so as to utilize its sensible heat, the æconomical duty may be at least doubled. In this case 150° out of 965° communicated to the boiler will be converted into force, leaving only 815° to be evolved in the condenser.

On De Vico's Comet. By the Rev. Prof. POWELL, F.R.S. &c.

The author called the attention of the Section to the expected return of this comet, the first since its discovery. It will come to its perihelion on Feb. 6, 1850, but will be in such a position with respect to the earth as probably to render it quite invisible. The annexed diagram gives a rough idea of its positions.

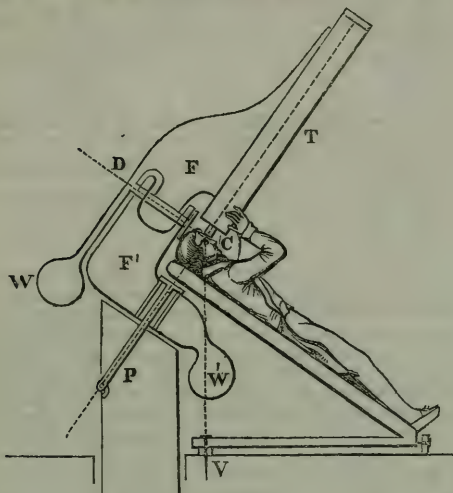


Prof. Chevallier has since printed an Ephemeris of the comet: the only period during which it could possibly be seen would be in October 1849.

*On a new Equatorial Mounting for Telescopes.
By the Rev. Prof. POWELL, F.R.S. &c.*

The object of the plan here proposed is mainly the personal convenience of the observer, and the ease and rapidity of changing to a new position, which is often a matter of more importance than mere comfort.

The essential principle of the construction (see the annexed section in the plane of the meridian) consists in making the telescope (T) move about a point (C), at, or a little beyond, its eye-hole, instead of, as usual, about its middle point, by means of a counterpoise (W). The telescope projects sufficiently from the frame (F) which



carries it to allow room for the head of the observer; and to this frame is attached the declination-axis (D), turning in supports from the lower frame (F'), through the arm of which passes the polar axis (P), fixed to a firm pier, and round which the whole is counterpoised by the weight (W').

When the whole is in the plane of the meridian the axis of the telescope coincides with the polar axis, and the point of intersection (C) of the polar and declination axes, is the position of the eye of the observer, or centre of motion, which is *an invariable point in space for all altitudes and azimuths*. A perpendicular dropped from this point gives a point (V) on the floor, at which a pivot is fixed, round which the observing couch (inclined at a constant angle) can revolve through a semicircle horizontally. The observer has only to push himself round in azimuth, and, in any given azimuth, he commands all altitudes in the plane of a circle at right angles to that azimuth, by simply turning his head from one side to the other.

The possible objection of want of steadiness in the telescope is one which can only be judged of by actual trial; but for telescopes of moderate size there seems no reason to apprehend that with well-constructed framing, axes, and counterpoises, sufficient steadiness might not be attained.

The same principle is obviously, and with greater simplicity, applicable to small transits and moveable telescopes. In the latter case it has been successfully put to trial by the author.

On the Friction of Water. By ROBERT RAWSON.

The object of this paper is to ascertain the friction of water on a vessel or other floating bodies rolling in water. For this purpose experiments have been made upon a cylindrical model whose length is 30 inches, diameter 26 inches, and weight 255·43 lbs. avoirdupois, in the following manner. The cylinder was placed in a cistern, in the first place, without water, and made to vibrate on knife-edges passing through the axis of the cylinder; a pencil projecting from the model in the direction of the axis of the cylinder on the surface of another moveable cylinder marked out upon paper placed upon this last cylinder the amplitude of each oscillation.

The cylinder was deflected over to various angles by means of a weight attached by a string to the arm of a lever fixed to the cylindrical model.

Angle of deflection.

°
22 30
22 10
21 54
21 36
&c.

Angle to which the model vibrated.

°
22 24
22 6
21 48
21 30
&c.

When the cylinder oscillated, in all circumstances the same as above, except being surrounded by salt water, the amplitude of oscillation was as follows:—

Angle of deflection.

°
22 30
21 36
20 48
&c.

Angle to which the model vibrated.

°
22 0
21 3
20 16
&c.

Clearly showing that the amplitude of vibration, when oscillating in water, is considerably less than when oscillating without water: in the above instance there is a falling off in the angle of amplitude of 24', or nearly half of a degree. This amount has been confirmed by several experiments made with great care; and it appears only fair to attribute this decrease in the amplitude of oscillation to the circumstance of the friction of the water on the surface of the cylinder. The amount of force acting on the surface of the cylinder necessary to cause the decrease in the amplitude of oscillation shown by the experiment was calculated, and the author thinks that this amount of force is not equally distributed on the surface of the cylinder: in consequence of this he thought the amount on any particular part might vary as the depth. On this supposition a constant pressure at a unit of depth is assumed; this, multiplied by the depth of any other point of the cylinder immersed in the water, will give the pressure at that point. These forces or moments being summed by integration and equated with the sum of the moments given by the experiments, we shall have the following value of the constant pressure at a unit of depth, .0000469. This constant in another experiment, the weight of the model being 197 lbs. avoirdupois, (and consequently the part immersed in the water was very different from the other experiment) was .0000452, which differs very little from the former; showing that the hypothesis assumed in computation is not far from the truth.

On Elliptic Integration. By ROBERT RAWSON.

The object of this communication is, in the first place, to change the form of the elliptic function from that involving the square of the sine of the amplitude to a form involving simply the cosine of the amplitude, by means of the well-known trigonometrical formula, that twice the sine of half an arc squared is equal to unity minus the cosine of the same arc.

The author believes this form of the elliptic function to possess several advantages, and therefore would be more useful to tabulate than the form of Legendre, whose tables are not in a good practical form for use. With a view to tabulate this function in a more extensive manner than Legendre has done, several investigations have been made to compare the functions of the first order with different amplitudes and moduli; a formula for this purpose has been obtained where the relation between the amplitudes is much more simple than that discovered by Lagrange.

A different mode of investigation has been pursued by the author than that pursued by Lagrange, Legendre, Abel, or any authors who have written on this subject; and by taking a relation between the amplitudes expressed by means of an unknown function of one of the amplitudes, we are conducted to two equations, the first of which is an elliptic function of the first order, equal to a constant times another integral of an arbitrary character, and the second a functional equation, which must be satisfied, between the function assumed in the relation of the amplitudes and

the function assumed in the arbitrary integral by means of which the elliptic integral is compared.

If we want to compare the elliptic function with another elliptic function of the same kind but a different modulus, the function in the arbitrary integral will be the same radical which enters in the elliptic integral; but if the object be to compare the elliptic function with any other integral of a different form, the function in the arbitrary integral will be fixed, and depend upon the form of the integral thus used.

Various rational forms have been given to this arbitrary integral, with a view to compare the elliptic functions with functions that can be integrated, and amongst them one has been found to answer the conditions of the functional equation, and also to be integrable by means of logarithms and circular arcs.

On the Oscillations of Floating Bodies. By ROBERT RAWSON.

This paper had for its object the description of a course of experiments made at Portsmouth Dockyard by Mr. John Fincham, the master shipwright, and the author, with a view to confirm several important formulæ discovered by Professor Moseley relative to the rolling and pitching motion of vessels. All the experiments, which were made by Admiralty order, confirm the formulæ for determining the amount of force or work done to deflect a floating body in a state of equilibrium through a given angle, and also another formula which determines whether the vessel thus deflected will move slowly or otherwise.

The importance of these questions to naval architecture is obvious; and all the experiments we have made show what we believe to be an important practical fact, viz. that when a sudden gust of wind is applied to the sails of a vessel, or any cause which acts constantly during one oscillation, the ultimate amplitude of deflection will be double the amplitude which the gust of wind will permanently deflect the vessel.

In the next part, several experiments were made on models of vessels, some of which have been built with a view to ascertain the best form of midship section which will give the easiest rolling motion.

Description of a Binocular Camera.

By Sir DAVID BREWSTER, K.H., D.C.L., F.R.S., & V.P.R.S. Edin.

This instrument affords to amateurs and artists a ready mode of obtaining double drawings both of colossal statues and of living bodies or of fixed structures, for the purpose of having them exhibited as solids by the stereoscope. As the camera required for this purpose must have two lenses of exactly the same focal length, in order to form by the Daguerreotype or Talbotype processes the two pictures required, with mathematical precision, Sir David has constructed his double camera by dividing a suitable lens, either single or achromatic, into two semi-lenses, each of which will form an image exactly like that which the entire lens would have formed, though with less light. These semi-lenses, placed at the proper distance from each other and from the object, give the two pictures as required for producing the effect of relief when seen by each eye at once in a stereoscope.

Improvement on the Photographic Camera.

By Sir DAVID BREWSTER, K.H., D.C.L., F.R.S., & V.P.R.S. Edin.

Sir David Brewster gave an account of an improvement which he had made and used on the Photographic Camera. In order to observe when the picture was most distinct on the paper or metal, he views the picture with a single or a compound eye-piece, either when the picture is received on the ground-glass plate, or in the air when the ground-glass is removed. In this last case the camera becomes an excellent telescope, by which the satellites of Jupiter as well as other astronomical phenomena may be easily seen. The ground-glass may be wholly dispensed with, or it may be permanently connected with the eye-piece, and drawn back when it is out of use. If the ground-glass is retained, a hole opposite the eye may be made in it, or that part of the glass may be left unground. This construction of the Camera, by which the focus can be adjusted with the greatest accuracy, has been adopted and successfully by Mr. Beickle of Peterborough, who has executed some of the finest Talbotype we have seen.

On a new form of Lenses, and their Application to the Construction of two Telescopes or Microscopes of exactly equal Optical Power. By Sir DAVID BREWSTER, K.H., D.C.L., F.R.S., & V.P.R.S. Edin.

This method is to divide, in the same way as before described, one lens into two semi-lenses or quadrants, or sextants or octants, and using each semi-lens or quadrant for forming the image. Sir David also showed, that by a proper combination and adjustment of two such semi-lenses or quadrants in a frame or tube, by placing their diameters at a proper angle, each may be made to correct the imperfect image formed by the other.

Notice of Experiments on Circular Crystals.

By Sir DAVID BREWSTER, K.H., D.C.L., F.R.S., & V.P.R.S. Edin.

Mr. Fox Talbot first studied the phenomena of this class of crystals as exhibited in those produced by a mixture of borax and phosphoric acid, and Sir David Brewster exhibited to the Section drawings of this phenomenon which had been presented to him by Mr. Fox Talbot. In the course of his own inquiries he discovered a large number of bodies which yielded circular crystals, which he divided into two classes, *positive* and *negative*, including oil of mace (the phenomena of which he had previously described in the Phil. Trans. for 1814); animal fat, wax, &c., in which it is very difficult to distinguish *circular* from *quaquaversus* polarization.

Additional Observations on Berkeley's Theory of Vision.

By Sir DAVID BREWSTER, K.H., D.C.L., F.R.S., & V.P.R.S. Edin.

In this paper, the author, by various arguments, partly metaphysical, partly optical, and by examining the accounts of persons couched for cataract at an advanced period of life, which were relied on by the supporters of Berkeley's views (illustrating his views by several diagrams), considered he had overthrown every position essential to the maintenance of that theory, and especially the fundamental proposition from which that philosopher started.

An Account of a new Stereoscope.

By Sir DAVID BREWSTER, K.H., D.C.L., F.R.S., & V.P.R.S. Edin.

The ingenious stereoscope, invented by Professor Wheatstone for representing solid figures by the union of dissimilar plane pictures, is described in his very interesting paper "On some remarkable and hitherto unobserved Phenomena of Binocular Vision;" and in a paper published in a recent volume of the Edinburgh Transactions, Sir David Brewster has investigated the cause of the perception of objects in relief, by the coalescence of dissimilar pictures. Having had occasion to make numerous experiments on this subject, he was led to construct the stereoscope in several new forms, which, while they possess new and important properties, have the additional advantages of cheapness and portability. The first and the most generally useful of these forms is the Lenticular Stereoscope. This instrument consists of two semilenses placed at such a distance that each eye views the picture or drawing opposite to it through the margin of the semilens, or through parts of it equidistant from the margin. The distance of the portions of the lens through which we look, must be equal to the distance of the centres of the pupils, which is, at an average, $2\frac{1}{2}$ inches. The semilenses should be placed in a frame, so that their distance may be adjusted to different eyes. When we thus view two dissimilar drawings of a solid object, as it is seen by each eye separately, we are actually looking through two prisms, which produce a second or refracted image of each drawing, and when these second images unite, or coalesce, we see the solid object which they represent. But in order that the two images may coalesce without any effort or strain on the part of the eyes, it is necessary that the distance of similar parts of the two drawings be equal to *twice* the refraction produced by each lens. For this purpose, measure the distance at which the semilenses give the most distinct view of the drawings, and having ascertained, by using one eye, the amount of the refraction produced at that distance, or the quantity by which the image of one of the drawings is displaced, place the drawings at a distance equal to twice that quantity,

that is, place the drawings so that the average distance of similar parts in each is equal to twice that quantity. If this is not correctly done, the eye of the observer will correct the error, by making the images coalesce, without being sensible that it is making any such effort. When the dissimilar drawings are thus united, the solid will appear standing, as it were, in relief, between the two plane representations of it. In looking through this stereoscope, the observer may probably be perplexed by the vision of *only the two dissimilar drawings*. This effect is produced by the strong tendency of the eyes to unite two similar, or even dissimilar drawings. No sooner do the refracted images emerge from their respective drawings, than the eyes, in virtue of this tendency, force them back into union; and though this is done by the convergency of the optic axes to a point nearer the eye than the drawings, yet the observer is scarcely conscious of the muscular exertion by which this is effected. This effect, when it does occur, may be counteracted by drawing back the eyes from the lenses, and shutting them before they again view the drawings. While the semi-lenses thus double the drawings and enable us to unite two of the images, they at the same time magnify them,—an advantage of a very peculiar kind, when we wish to give a great apparent magnitude to drawings on a small scale, taken photographically with the camera. The lenticular stereoscope may be made of any size. Sir David Brewster then described how we may see at the same time a *raised* and a *hollow* cone, the *former* being produced by the union of the *first* with the *second*, and the latter by the union of the *second* with the *third* figures. This method of exhibiting at the same time the raised and the hollow solid, enables us, he said, to give an ocular and experimental proof of the usual explanation of the cause of the large size of the horizontal moon, of her small size when in the meridian at a considerable altitude, and her intermediate apparent magnitude at an intermediate altitude. As the summit of the raised cone *appears* to be nearest the eye of the observer, the summit of the hollow cone furthest off, and that of the flat drawing on each side at an intermediate distance, these distances will represent the apparent distance of the moon in the zenith of the elliptical celestial vault, in the horizon, and at an altitude of 45° . The circular summits thus seen are in reality exactly of the same size, and at the same distance from the eye, and are therefore precisely in the same circumstances as the moon in the three positions already mentioned. If we now contemplate them in the stereoscope, we shall see the circular summit of the hollow cone the *largest*, like the *horizontal* moon, because it seems at the *greatest* distance from the eye; the circular summit of the *raised* cone the *smallest*, because it appears at the *least* distance, like the *zenith* moon; and the circular summit of the cones on each of an *intermediate* size, like the moon at an altitude of 45° , because their distance from the eye is intermediate. No change is produced in the apparent magnitude of these circles by making one or more of them less bright than the rest, and hence we see the incorrectness of the explanation of the size of the horizontal moon, as given by Dr. Berkeley. When the observer fails to see the object in relief from the cause already mentioned, but sees only the *two* drawings, if there are *two*, or the *three* drawings, if there are *three*, the plane of the drawings appears *deeply hollow*; and, what is very remarkable, if we look with the eccentric lenses at a flat table from above, it also appears deeply hollow, and if we touch it with the palm of our hand, *it is felt as hollow*, while we are looking at it, but the sensation of hollowness disappears on shutting our eyes. Sir David Brewster described a variety of forms in which he had constructed the stereoscope, by means of lenses, mirrors and prisms. The sense of sight, therefore, instead of being the pupil of the sense of touch, as Berkeley and others have believed, is, in this as in other cases, its teacher and its guide. Sir D. Brewster's simplified stereoscopes may not only be rendered portable, but may be constructed out of materials which every person possesses, and without the aid of an optician. A fuller account of these instruments will be found in the forthcoming volume of the Transactions of the Royal Scottish Society of Arts.

Experiments on the Inflection of Light. By LORD BROUGHAM, F.R.S.

A communication from Lord Brougham was read by Sir David Brewster. His Lordship's experiments were made at his seat at Cannes in Provence, with a very fine

and ingenious apparatus executed by that distinguished optician, M. Soleil of Paris, and with the aid of a heliostat for fixing the sun-beam in one position during the day. The results obtained by Lord Brougham establish a new and interesting property of light, namely, that when a pencil of divergent light has suffered inflection by a metallic or any other edge, of any form or substance, it exhibits different properties on its different sides when submitted to the action of a second inflecting edge. The heliostat being a rare and expensive instrument, and of difficult construction, Lord Brougham offered the use of his to any members of the Association who might be occupied with experiments on light which required the assistance of it.

On the Diurnal Variation of Magnetic Declination and the Annual Variation of Magnetic Force. By J. A. BROWN.

The details of these results will be found in vol. xix. part 2 of the Transactions of the Royal Society of Edinburgh.

On an Orbital Motion of the Magnetic Pole round the North Pole of the Earth. By the Rev. H. M. GROVER.

This subject was investigated by tracing the positions of the magnetic pole at several intervals during the period of the last 250 years, by converging lines drawn from the London, Paris and St. Petersburg Observatories, and deduced by computations of the different variations of the magnetic needle at these places. These changes were shown very distinctly upon the different polar horizons of the observatories, and the orbit drawn from them in its proper position. An extraordinary acceleration of this motion from 1580 down to 1723 was pointed out, and a pause at that period, which indicated a climax in that year, in which both the horizontal movement of the needle was suspended, and the dipping motion changed its course from a downward to an upward motion. Mr. Grover showed also a series of changes in the lines of equal declination about the isodynamic poles, which appeared to indicate a direct tendency, or attractive force operating upon the magnetic needles from those poles, which he assumed and showed to be sufficient to account for the extra linear position of the line of no declination between Europe and Asia, as well as for the extraordinary curvatures of the declination lines observed in the north of Asia on the two sides of the isodynamic pole, and the origin and changes of the closed systems or ovals in their Asiatic and Pacific allocations. Mr. Grover regarded the moving magnetic pole in the light of a satellite, or supplemental system, to the isogonal poles, disturbed by the accumulations of ice about the pole in the course of a long series of ages, and generated as a compensative process from an interruption of the original system.

On some recent Discussions relative to the Theory of the Dispersion of Light. By the Rev. Prof. POWELL, F.R.S. &c.

Two eminent continental writers have recently published some discussions on this subject, which seem to call for a few brief remarks.

M. Mossotti, in a memoir "On the Spectrum of Fraunhofer," &c. (Paris, 1845, transl. in Taylor's Foreign Scientific Memoirs, No. xix. p. 435), compares the interference or grating spectrum, with that formed by refraction, as to the intensity of light at its different parts, and the relation of the deviation to the values of λ , the wave length, which in the former is simple and normal.

As to any measures of the intensity of light at different parts of the spectrum, it appears to be a necessary condition to state the kind or degree of light used, whether the full solar rays, or bright or dull daylight; since in the former case the intensity of the middle part of the spectrum enormously exceeds and overpowers that of every other part, while in the latter cases it is only in a slight degree brighter.

In Part II. § 2, the author gives the dispersion formula substantially the same as M. Cauchy's, as far as the 4th power of λ . He adopts a method of calculation by means of least squares, and considers the verification sufficient, from the agreement in one specimen of Fraunhofer's flint-glass.

It cannot but excite surprise to find so eminent a philosopher at the present day

referring to the ideal analogy of musical intervals. Yet Newton's notion of the permanency of the proportions in the analysis of light might seem but the necessary consequence of the permanence of the synthetic result. And this main difficulty yet remains to be cleared up.

The Abbé Moigno has also referred more particularly to this subject in his 'Répertoire d'Optique Moderne' (Paris, 1847, vol. i. p. 123-126). After giving M. Cauchy's formula, he states that in my comparisons of observation and theory for a great number of media the differences never exceeded the probable errors of observation. Unfortunately not only is this not the case, but I have expressly dwelt upon it, in reference to one or two most highly dispersive substances. He also observes, that M. Cauchy (in his 'Nouveaux Exercices') has re-calculated the results, and shown a perfect accordance. But this applies only to Fraunhofer's ten media, and does not extend to the highly dispersive oils. He further states the deduction from M. Cauchy's formula, that the differences of the squares of velocities of propagation are very nearly as the reciprocals of the squares of the wave-lengths; but adduces only one case of flint-glass in proof. It seems to be often overlooked, that though there is a close accordance for all moderately dispersive substances, yet a very few instances to the contrary in the higher part of the scale suffice to show that the formula stands in need of some essential modification to make it apply to them, while it shall still include the former. I have pointed out* that an empirical constant for each medium will rectify the discordance. Whether this can be justified by theory, is the point to which the attention of mathematicians ought I think now to be directed.

Both M. Mossotti and M. Moigno admit the necessity of assuming (according to the number of terms taken) three or four experimental constants for the medium. I notice this, because it has been objected to in my investigations. But on distinct grounds it appears to me evident that from the nature of the problem we must suppose at least three constants to characterize each medium. In other words, the problem is a compound one, each medium having as it were three distinct properties:—1st, the absolute magnitude of its refraction, or deviation of the whole spectrum or of a mean ray; 2nd, the *magnitude* of dispersion, or expansion of the whole; 3rd, the *character* of the dispersion or relative expansion of the parts: conditions which are certainly independent of each other, and each of which would involve a separate constant.

On Irradiation. By the Rev. Prof. POWELL, F.R.S. &c.

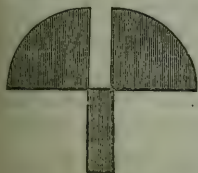
The phenomenon known by the name of Irradiation is best exhibited by the method of M. Plateau, which forms the basis of all the author's experiments, and which consists of a card or lamina cut so that a long parallelogram has one half cut out and the other left, the portions at the sides being cut away. Thus the effect is seen doubled either by transmitted or reflected light. [In the annexed sketch the shaded portions represent the parts cut away.] It is well established that the effect increases with the intensity of the light. It is also evident that it decreases rapidly towards the edge of the enlarged surface.

The effect has been ascribed by most writers to a peculiar kind of physiological affection of the retina. But (allowing for the effects of dazzling, contrast, &c.) the author has shown that this is not the case, since exactly the same effect is produced in an artificial eye, or camera obscura. The effect has also been tried photographically, in some cases especially in direct sunlight, with perfect success; in others without effect. But the most effective photographic rays are not the most illuminating, and may therefore not be equally subject to this modification.

These phenomena appear to be simply cases of the enlarged focal image of a luminous point, which is a well-known result, both of theory, as investigated by Mr. Airy†, and of observation, as seen in the discs of fixed stars under contracted apertures.

* Treatise on Dispersion, &c., p. 119.

† Camb. Trans. v. 283.



The effect on the eye is diminished, and may be totally destroyed, by the intervention of a *lens*, even in the brightest lights. This is explained by the diminution of intensity in proportion to the superficial magnification, which is most effective at the edges.

In telescopes there is a twofold effect of this kind, one at the focus of the eye, another at that of the object-glass; the former may be neutralized by the magnification of the eye-piece. The author has tried many experiments on the image of a card, cut as above, seen in a telescope under apertures of various degrees of contraction, which appear to accord closely with the phenomena of "the diffraction of the object-glass." It also follows that there must be a limit to the increase of the enlargement of the image, dependent on the diminution of light when the aperture is contracted beyond a certain point, which will vary in each individual instrument.

The author suggests a method of measuring the amount of irradiation under any given conditions of light, by viewing and measuring micrometrically in a telescope the image of a card cut as above, under the given light, placed at the focus of an object-glass opposite to that of the telescope, and connected with it by a tube.

Theoretically, irradiation would explain those singular phenomena seen in eclipses and transits of the planets, of the connection of the edge of the dark disc by *necks* or *threads* to that of the sun; as also the apparent projection of a star on the *bright* limb of the moon, by simply overlapping the star from irradiation. But the difficulty in all these phenomena is their appearance in some cases and not in others, under circumstances apparently similar.

On a Mode of Measuring the Astigmatism of a Defective Eye.

By Professor STOKES, M.A.

Besides the common defects of long sight and short sight, there exists a defect, not very uncommon, which consists in the eye's refracting the rays of light with different power in different planes, so that the eye, regarded as an optical instrument, is not symmetrical about its axis. This defect was first noticed by the present Astronomer Royal, in a paper published about twenty years ago in the Transactions of the Cambridge Philosophical Society. It may be detected by making a small pin-hole in a card, which is to be moved from close to the eye to arm's length, the eye meanwhile being directed to the sky, or any bright object of sufficient size. With ordinary eyes the indistinct image of the hole remains circular at all distances; but to an eye having this peculiar defect it becomes elongated, and, when the card is at a certain distance, passes into a straight line. On further removing the card, the image becomes elongated in a perpendicular direction, and finally, if the eye be not too long-sighted, passes into a straight line perpendicular to the former. Mr. Airy has corrected the defect in his own case by means of a spherico-cylindrical lens, in which the required curvature of the cylindrical surface was calculated by means of the distances of the card from the eye when the two focal lines were formed. Others however have found a difficulty in preventing the eye from altering its state of adaptation during the measurement of the distances. The author has constructed an instrument for determining the nature of the required lens, which is based on the following proposition:—

Conceive a lens ground with two cylindrical surfaces of equal radius, one concave and the other convex, with their axes crossed at right angles; call such a lens an *astigmatic lens*; let the reciprocal of its focal length in one of the principal planes be called its *power*, and a line parallel to the axis of the convex surface its *astigmatic axis*. Then, if two thin astigmatic lenses be combined with their axes inclined at any angle, they will be equivalent to a third astigmatic lens, determined by the following construction:—Through any point draw two straight lines, representing in magnitude the powers of the respective lenses, and inclined to a fixed line drawn arbitrarily in a direction perpendicular to the axis of vision at angles equal to *twice* the inclinations of their astigmatic axes, and complete the parallelogram. Then the two lenses will be equivalent to a single astigmatic lens, represented by the diagonal of the parallelogram in the same way in which the single lenses are represented by the sides. A plano-cylindrical or spherico-cylindrical lens is equivalent to a common

lens, the power of which is equal to the semi-sum of the reciprocals of the focal lengths in the two principal planes, combined with an astigmatic lens, the power of which is equal to their semi-difference.

If two plano-cylindrical lenses of equal radius, one concave and the other convex, be fixed, one in the lid and the other in the body of a small round wooden box, with a hole in the top and bottom, so as to be as nearly as possible in contact, the lenses will neutralize each other when the axes of the surfaces are parallel; and, by merely turning the lid round, an astigmatic lens may be formed of a power varying continuously from zero to twice the astigmatic power of either lens. When a person who has the defect in question has turned the lid till the power suits his eye, an extremely simple numerical calculation, the data for which are furnished by the chord of double the angle through which the lid has been turned, enables him to calculate the curvature of the cylindrical surface of a lens for a pair of spectacles which will correct the defect of his eye.

On the Determination of the Wave Length corresponding with any point of the Spectrum. By Professor STOKES, M.A.

Mr. Stokes said it was well known to all engaged in optical researches that Fraunhofer had most accurately measured the wave lengths of seven of the principal fixed lines in the solar spectrum. Now, he found that by a very simple species of interpolation, which he described, he could find the wave lengths for any point intermediate between two of them. He then exemplified the accuracy to be obtained by his method by applying it to the actually known points, and showed that in these far larger intervals than he ever required to apply the method to the error was only in the eighth, and in one case in the seventh place of decimals. By introducing a term depending on the square into the interpolation still greater accuracy was attainable. The mode of interpolation proposed depended upon the known fact, that, if substances of extremely high refractive power be excepted, the increment $\Delta\mu$ of the refractive index in passing from one point of the spectrum to another is nearly proportional to the increment $\Delta\lambda^{-2}$ of the squared reciprocal of the wave length. Even in the case of flint-glass, the substance usually employed in the prismatic analysis of light, this law is nearly true for the whole spectrum, and will be all but exact if restricted to the small interval between two consecutive standard fixed lines. Hence we have only to consider μ as a function, not of λ , but of λ^{-2} , and then take proportional parts.

On examining in this way Fraunhofer's indices for flint-glass, it appeared that the wave length ($B\lambda$) of the fixed line B was too great by about 4 in the last, or eighth, place of decimals. It is remarkable that the line B was not included in Fraunhofer's second and more accurate determination of the wave lengths, and that the proposed correction to ($B\lambda$) is about the same, both as to sign and magnitude, as one would have guessed from Fraunhofer's own corrections of the other wave lengths, obtained from his second series of observations.

On Professor Quetelet's Investigations relating to the Electricity of the Atmosphere, made with Peltier's Electrometer. Communicated by Professor WHEATSTONE, F.R.S.

Of all the meteorological conditions of the atmosphere its electrical state is perhaps among the most important. Yet in the various observatories established in different parts of the world in connexion with the great magnetic inquiry now in progress, and in the establishment of which the British Association has taken so prominent a part, no provision has been made for regular observations relating to this important subject. It thus happens, that while we possess a most valuable accumulation of periodical records, made with great accuracy and regularity at widely different points of the earth's surface, relating to the magnetism of the earth, and to the barometric, thermometric, hygrometric, and anemometric conditions of the atmosphere, we have no simultaneous electrical observations with which to compare them.

This has arisen from the want of a simple and efficient instrument by which such observations could be made. The most valuable results which have hitherto been obtained have been made with fixed electric apparatus. That established at the Observatory of the British Association at Kew, under the superintendence of Mr. Ronalds, and in which he has introduced so many important improvements which render it, in perfectness of insulation, and the comparability of its attached electrometers, superior to any hitherto erected, will no doubt, when the observations made at the establishment during the past five years are reduced and discussed, as is now being done by Mr. Birt, yield valuable results. Still such apparatus are too costly, and require too many precautions in their establishment and manipulation to be recommended for general use.

Meteorologists will therefore learn with satisfaction that this deficiency is now supplied by the late M. Peltier's induction electrometer, a portable instrument, simple in its construction, certain in its results, and of which any number may be made perfectly comparable with each other. One of these instruments is on the table. A hollow ball of copper, four inches in diameter, is placed at the top of a rod of the same metal, which is terminated at its lower extremity by a much smaller ball. From the last-mentioned ball, insulated from the glass cover by a lump of shell-lac, descends a copper rod, which bifurcates and forms a kind of ring. At the centre of this ring a small copper needle, which forms the essential part of the instrument, moves freely, balanced on a pivot. When the electrometer is in its natural state, the needle is brought to the magnetic meridian by a much smaller magnetic needle which is parallel to it, and fixed immediately above it. Another copper needle, much thicker than the moveable one, forms a system with the rod which descends into a glass tube filled with shell-lac and fixed into the wooden stand. Thus the entire metallic part of the apparatus is insulated, and electricity can be communicated from it neither to the glass cover nor the stand. This insulation must be established with the greatest care. The stand is furnished with three leveling screws, which enables it to be placed horizontally. To prepare the instrument for an observation, it must be so placed that the fixed needle shall be in the direction of the magnetic meridian. In this position, the moveable needle, directed by its small magnetic needle, places itself parallel to the fixed needle. If now a body electrified, positively or negatively, be held above the ball, it decomposes by induction the electricity of this ball and its metallic appendages. If the body be positively electrified, at the upper extremity of the ball the negative electricity is coerced by the positive electricity in presence, while in the lower part of the instrument the free positive electricity causes the small needle to diverge from the position which it had at first, and its angle of deviation from the fixed needle will be greater as the free electricity is more considerable. The angle of deviation is read off by means of two graduated circles, one of which is pasted to the stand and the other to the glass cover; by this parallax is avoided in the readings. If while the ball is influenced by the external electricity, the stem is touched, the free electricity, which we will assume to be positive, will be removed and the needle will replace itself in the magnetic meridian. If the inducing body which coerces the negative electricity at the upper part of the ball be removed immediately after, this electricity will become free, and the moveable needle will diverge anew.

I will state in M. Peltier's own words the mode of operating with this instrument when the electricity of the air is to be observed. "When I wish to ascertain the electric tension accumulated in the atmosphere, I ascend to the terrace, I place the instrument on a stand raised about 6 feet, I put it in equilibrium by touching the lower part of the stem, I then descend, and place the instrument on the table appropriated to it: all this is performed with great rapidity, and requires only eight seconds: when the instrument is put in equilibrium, the arm of the observer must be raised as little as possible, for if it be raised sufficiently to reach the globe, the hand becoming negative by induction will repel the negative electricity of the ball; it will neutralize the positive portion which will be attracted towards it, and the instrument will be charged negatively at the moment of the removal of the hand. The stem must therefore be touched as low as possible, and with as slender a body as a metallic wire, in order to avoid the inductive action of the mass of the hand upon the remainder of the stem. The equilibrium being established when it is

elevated at any height above the surface, the instrument on being lowered gives signs of negative electricity, while on being raised it will indicate positive. When the operation is thus performed, this change of sign must be taken into consideration, in order not to attribute a contrary electricity to the atmosphere. In like manner a negative tension of the atmosphere is indicated, when the instrument after descending into the cabinet gives a positive sign."

The proportional forces corresponding to the marked degrees of Peltier's electrometer may be ascertained in two ways. Peltier has given a table, in which the equivalent degrees were determined by an electric torsion-balance; but the method employed by Quetelet is more easily applicable, and gives very satisfactory and comparable results. This method, which is that of Volta, consists in dividing the electric charges by placing spheres of the same diameter in contact. He took two electrometers, each surmounted by a metallic ball of the same diameter; he commenced by charging the first electrometer so that the needle indicated $74^{\circ}5$, the two balls were then placed in contact to divide the charge. After this first operation the electrometer indicated only 70° . Their values, according to Peltier's table, correspond to 2825° and 1400° of the torsion-balance, which corresponds almost exactly with the ratio of 2 : 1. After having discharged the second electrometer, he again placed it in contact with the first, which this time only indicated 64° , or 795° of the table of equivalents, which is nearly half of the number 1400. This operation was repeated several times in succession in order to form the table.

A regular and uninterrupted series of observations has been made with this instrument by M. Quetelet at the Royal Observatory in Brussels since the beginning of August 1844. He has recently published these observations, extending through four and a half years, from this date till the 31st of December 1848, and the consequences he has deduced from them are very satisfactory and important. I will briefly state the principal of these results, referring for more extended details to the last memoir he has published on the climate of Belgium*.

The first object of M. Quetelet was to ascertain the relation that exists, under ordinary circumstances, between the different heights above the neutral point and the electric intensities. The experiments of Erman and Saussure had long since made known that electricity is not equally expanded in the atmosphere; that it is nearly of the same intensity in a horizontal stratum of air, and stronger in the upper strata. The discussion of the numerous experiments made by M. Quetelet with respect to this point, shows that in a place in the neighbourhood of which there are no higher objects, the electric intensity of the air increases, starting from a determinate point, proportionally to the height. But it must be borne in mind that this law has only been verified with respect to heights not exceeding 16 feet.

The observations, with the view of ascertaining the annual variations of atmospheric electricity, were made every day at about the hour of noon, commencing in August 1844. The results of each year are in complete concordance, and are as follows:—1st. The atmospheric electricity, considered in a general manner, attains its maximum in January, and progressively decreases till the month of June, which presents a minimum of intensity; it augments during the following months till the end of the year. 2ndly. The maximum and the minimum of the year have for their respective values 605 and 47; so that the electricity in January is thirteen times more energetic than in the month of June. The mean value of the year is represented by the values given by the months of March and November. 3rd. The absolute maxima and minima of each month follow a course precisely analogous to that of the monthly means; the means of these extreme terms equally produce the annual variation, although in a less decided manner.

In order to determine the intensity of the electricity of the air in its relations with the state of the sky, M. Quetelet separated, for each month of the year, the numbers which referred to a sky entirely clouded, from those observed when the sky was serene, or rather presenting so few clouds that eight- or nine-tenths were at least entirely unclouded. In order not to complicate the results by foreign influences, he omitted the observations made during storms, snow, rain and fogs. The table thus formed gave the following results:—1st. Whatever be the state of the sky,

* *Annales de l'Observatoire Royale de Bruxelles*, tom. vii. 1849.

the electricity of the air presents a maximum in January and a minimum towards the summer solstice. 2ndly. The difference between the maximum and the minimum is much more sensible in serene than in cloudy weather. In the latter case the numbers are 268 and 36, the ratio of which is about 7. In serene weather the maximum of January is 1133° and the minimum of July 35° ; the ratio of these numbers is 36, which shows a very considerable difference. 3rdly. Throughout every month the electricity of the air is stronger when the sky is serene than when it is clouded, except towards the months of June and July, when the electricity attains a minimum, the value of which is nearly the same whatever be the state of the sky.

Starting from this epoch, the electricity of the air, when the sky is clear, exceeds the electricity observed when the sky is entirely clouded, in proportion as the months advance towards January, when the ratio is more than 4 to 1. This strong electric intensity, under a clear sky in winter, is a very remarkable circumstance, and had already been noticed by all the investigators of atmospheric electricity, although they attributed to it a much less relative value.

Monthly variations in the Electricity of the Air.

	1844.	1845.	1846.	1847.	1848.	Mean.
January	471	562	957	487	605
February	548	256	413	295	378
March	262	95	282	164	200
April	93	94	221	155	141
May	163	49	67	59	84
June	51	39	47	48	47
July	58	33	43	61	49
August	90	89	57	11	64	62
September...	91	95	62	39	63	70
October	110	299	98	107	120	131
November ...	127	334	274	160	152	209
December ...	340	742	799	356	281	507
Annual mean	...	267	202	225	162	206

Electricity of the Air in relation to the state of the Sky.

	J.	F.	M.	A.	M.	J.	J.	A.	S.	O.	N.	D.
Clouded sky...	268	220	129	71	46	36	41	56	42	75	109	181
Clear sky.....	1133	493	261	149	63	37	35	64	78	168	226	571

M. Quetelet has united in a special table the observations made during extraordinary circumstances, such as fogs, snow showers and rain, and which he did not employ in the calculation of the means. From this table he has obtained the following results :—The mean of the electric intensity observed during fogs is almost exactly the same value as that observed during snow showers; this value is very high, and corresponds to the mean maxima observed for the first and last months of the year. It does not appear that it is influenced by the seasons. The values observed during tranquil rain do not differ much from the ordinary values observed during the course of the year. In some circumstances a strong electricity, either positive or negative, has been observed at the approach or cessation of rain. During the four years included in M. Quetelet's register, the electricity at the ordinary hour of observation was observed to be negative only twenty-three times; and he remarks that it was only observed to be negative once during the four months of October, November, December and January. These negative electric indications in general precede or follow rain and storms; they are thus distributed: the electricity has been observed to be negative six times during rain, nine times before rain, five times after rain, twice during rain falling in distant places, once without apparent cause.

From the observations made to ascertain the diurnal variation of the electricity of the air, M. Quetelet deduces the following conclusions:—1st. The electricity of the air, estimated always at the same height, undergoes a diurnal variation, which generally presents two maxima and two minima. 2ndly. The maxima and minima vary according to the different periods of the year. 3rdly. The first maximum occurs, in summer, before eight o'clock in the morning, and towards ten o'clock in winter; the second maximum is observed after nine o'clock in the evening in summer, and towards six o'clock in winter. The interval of time which separates the two maxima is therefore more than thirteen hours at the epoch of the summer solstice, and eight hours only at the winter solstice. 4thly. The minimum of the day presents itself towards three o'clock in the summer, and towards one o'clock in winter. The observations were insufficient to establish the progress of the night maximum. 5th. The instant which best represents the mean electric state of the day, in the different seasons, occurs about eleven o'clock in the morning.

The indications afforded by Peltier's electrometer are simpler and more readily interpreted than atmospheric electrometers of the usual construction. The former is affected only by the inductive action of the atmosphere, or rather by the difference of the inductive actions of the earth and its superincumbent atmosphere; however the instrument be raised above or depressed below its point of equilibrium, or however the inductive action of the atmosphere may change, while it remains in the same position it neither receives nor loses any electricity; its distribution only is changed. But if instead of a polished ball the stem be terminated with a point, a bundle of points, or a lighted wick, as in Volta's experiments, to the phenomenon of induction there is added another which complicates and sometimes disguises it; the uncoerced electricity radiates into space, and though this radiation is greater as the induction is more powerful, yet it is also greatly influenced by the moisture of the air, rain, and the force of the wind, none of which circumstances affect in any obvious degree the induction electrometer.

On Shooting Stars. By W. R. BIRT.

See Reports in this volume, page 1.

Meteorological Phenomena observed in India from January to May 1849.
By GEORGE BUIST, LL.D., F.R.S. (Communicated by Colonel SYKES.)

The papers comprised pressure curves of the barometer for five years at Bombay, four years at Madras, and four years at Calcutta; a map of the occurrence of storms at various places in India between the 19th and 25th of April 1849; corresponding observations at various places during storms in India on the 15th and 22nd of January; the same between the 20th and 23rd of February and the 1st and 3rd of May. The pressure of papers in the Section disabled Colonel Sykes from giving more than a running commentary upon the different phenomena. He called the attention of the Section to the general uniformity of the several pressure curves at the three presidencies in India; the maximum pressure being in December or January, and the minimum pressure in June or July; the *absolute* height of the barometer however was different in different years; but the gradual descent of the curve from January to June and ascent from June to January was rarely interrupted, excepting at Bombay in the months of September and October 1845 and 1846; in the months of August and September, at Madras, in 1841; and in November and December 1843 and 1844. At Calcutta the descent and ascent of the curves did not show any interruption, but the barometer appeared to have a greater annual range at Calcutta than at Bombay or Madras. Colonel Sykes called attention to the fact that these curves were not affected by the passage of the sun twice annually over the places of observation, nor by the occurrence of the monsoon at Calcutta and Bombay in June, and at Madras in October. The map of storms between the 19th and 24th of April, showed that they occurred almost simultaneously in the Punjab, near Wuzerabad, at Loodiana, at Simla, Delhi, Calpee, Alahabad, Calcutta, Bombay, Belgaum, Madras, and down the coast to Tranquebar; at Mangalore, and down the Malabar coast to Cochin, and on the western side of Ceylon.

Accompanying this map, Dr. Buist gave curves of horary oscillations from the 19th to the 25th of April at Bombay, Madras, Aden, Calcutta, Lucknow and Mangalore, and at none of these places were the daily oscillations of the atmosphere, with their two maxima and two minima, in the slightest degree interrupted; and with reference to the uniformity of these horary oscillations and the annual maximum and minimum pressure, Colonel Sykes called the attention of the Section to the singular coincidence of these movements of the atmosphere; with similar movements, at nearly the same hours and periods, of the electric intensity, as determined by Mr. Birt in a paper recently read by him in the Section. In the storms of the 15th and 22nd of January, 20th to 23rd of February, and 1st to 3rd of May, Dr. Buist gives the *simultaneous* reading of the maximum and minimum pressure of the barometer at various places. These readings show that the horary oscillation at places on the level of the sea may have different ranges; for instance, at Calcutta and Bombay, on the 18th and 19th of February, the horary oscillation at Bombay is respectively 0.104 and 0.132, and at Calcutta 0.159 and 0.165. Carrying the comparison to Aden, the discrepancy is yet greater—0.072 and 0.081. Similar instances occur at the other periods. In the meteorological crisis of the 15th of January, Dr. Buist considered that the storm was felt all over India; and amongst other places, where it fell severely, he mentions Jaunah in the Deccan, where, on the 14th of January 1849, there was a hail-storm, the hailstones being lenticular, and from two to two and a half inches in diameter, and weighing from one to two ounces each! On the whole, Dr. Buist is of opinion that meteorological disturbances extend over very considerable areas. Dr. Buist's papers were not accompanied by tables of temperature or moisture.

On a Rainbow seen after actual Sunset. By the Rev. Prof. CHEVALLIER.

The rainbow was seen at Esh, six miles west of Durham. The latitude of the place, determined by Bessel's method of observing the transits of stars over the eastern and western prime vertical, is $54^{\circ} 47' 25''$; and its longitude $6^m 45^s$ west. The elevation above the sea is 700 feet. The time of the setting of the sun's upper limb could not be observed, in consequence of clouds; but the computed time, allowing $33'$ for the horizontal refraction, was $8^h 36^m 2^s$. At $8^h 31^m 43^s$ the bow seemed to be a portion of an arc greater than a semicircle, approaching to the form of a Saracenic arch, both sides being visible to an elevation of about 40° . At $8^h 34^m 43^s$ the southern end had faded; but at the northern end the primary and secondary bow were both visible at an altitude of about 5° , the sky being sensibly darker between the two bows. This northern end of the rainbow continued visible until $8^h 37^m 48^s$, or $1^m 42^s$ after complete sunset; and at $8^h 38^m 43^s$, or $2^m 41^s$ after sunset, an irregular portion of the southern part of the bow was visible at an altitude of about 15° .

The time was accurately obtained by comparing the watch with a transit-clock immediately after the observation. The barometer at the time, at the Observatory at Durham, 347 feet above the level of the sea, stood at 29.48, the attached thermometer 61° , the external thermometer $57^{\circ}.5$, the wind S.W., force 4. In order to account for this appearance, it seems necessary to suppose either that the horizontal refraction was much greater than its ordinary value, or that the rainbow was formed in a very elevated region of the atmosphere.

Notices of Mirage on the Sea Coast of Lancashire. By T. HOPKINS.

In this paper Mr. Hopkins represented that he had observed the phenomenon called Mirage on certain parts of the sea coast of Lancashire, and had at different times examined the state of the atmosphere on various parts of the shore, but more particularly near Southport. Here he found that whilst the sky was cloudy, apparently threatening rain, evaporation in the air near the surface of the wet shore was very active; but at other times, when the sun was shining brightly, evaporation at the same short distance from the surface was checked or entirely stopped, and at such times mirage might be seen.

On the morning of July 9th mirage appeared at a certain distance to the north of

the spectator over the flat sandy shore, and on examining the state of the locality where the phenomenon had been seen, the following facts were ascertained:—

The temperature on the adjoining dry sand-hills was	87°
... on the moist sand of the flat shore	78°·1
... of a dry-bulb thermometer in air ...	65°·5
... of a wet-bulb thermometer in air ...	63°·6
Difference between the two last.....	1°·9

To account for these facts, Mr. Hopkins said that when mirage appeared the sun was shining brightly, and by his direct rays raised the temperature of the ground considerably, when energetic evaporation from the wet sandy shore took place, which sent much vapour into the atmosphere. The presence of this vapour in the air checked evaporation from the wet-bulb thermometer, and prevented it from becoming much cooled, and the wet-bulb thermometer at the same time, by the feebleness of its evaporation, proved the existence of the large amount of vapour in the locality. Now as mirage appeared only when the sun produced a large amount of vapour from the moist surface of the ground, which vapour was shown to be present by the state of the wet-bulb thermometer, it is to be inferred that the vapour caused the appearance of mirage.

It might be that some of the vapour was condensed by the comparatively cool air, at a small distance from the surface of the sand, and thus a stratum of cloud was formed from the surface of which light was reflected. But however this may be, the presence of vapour sufficient to saturate or nearly saturate the air in the part always accompanied the appearance of the mirage, and therefore is presumed to be the cause of it. Objects that were beyond the place where the mirage appeared were reflected by it as if they were reflected by water. Refraction sometimes accompanies mirage, distorting the reflected as well as other objects, nearer to the spectator than the mirage; but the refraction is quite a separate phenomenon, sometimes appearing with and sometimes without the mirage.

Mr. Hopkins exhibited a number of tabulated observations in corroboration of what had been advanced. He also said that recently, at Blackpool, in the middle of the day, with a clear sky and a strong sun, while the dry- and wet-bulb thermometers on the wet sandy shore were at the same height (70°), there was a difference of 5° between the two instruments on the adjoining cliffs, about sixteen yards high. These facts, Mr. Hopkins contended, proved, that while evaporation saturated the air near the surface and produced mirage, the atmosphere at the height named was comparatively dry, allowing evaporation to take place with considerable energy from the wet-bulb thermometer.

*Letter from Sir ROBERT H. INGLIS, Bart., F.R.S., to Col. SABINE, R.A.,
Aug. 8, 1849.*

We were at Gais (Canton Appenzell, Switzerland) a few days ago, and saw there, what may be familiar to you and other men of science, but was quite new to me and to the people at the place. About 3 P.M. on the 8th of August my servant called to me, "that there was something falling very curious." I went out—to the bridge which connects the old and new buildings of the Hotel du Bœuf,—and under the shade of the new house looked up and saw thousands and thousands of brilliant white motes, like snow, falling as in flakes. There were no clouds, but there was a kind of halo round the sun, or rather, as I looked up, there were in that direction apparently more and larger masses through which the rays passed; balls separated themselves, consisting of vast numbers, and these resolved themselves into fragments and came whirling and floating about. The master of the Hotel, M. Heen, joined us: he had obviously never seen anything of the kind before, and called out, "Des millions, des millions." He summoned his people to look. I continued to gaze till I was half-blinded. At first the fragments seemed to melt; and to the last I could distinguish no appearance of an animal. Our servant fancied that he saw something like wings; I certainly looked till, to my eye, they seemed to evaporate, but their disappearance and perhaps the re-appearance of the same individual, might have been owing to their turning at right angles instead of exhibiting their extent lengthways, and *vice versa*. This lasted—at least I looked—25 minutes. Cer-

tainly none came to the ground. Reaumur 20°, no wind. Gais is 3100 feet above the sea.

Of analogous facts (more or less so) Sir John F. W. Herschel, Bart. F.R.S., says, in a letter to Col. Sabine, R.A., I can mention two :—

1. In or about the year 1821, I remember seeing in Sir James South's telescope at Blackman Street, when turned in a direction near, but not *to* the sun, about midday, frequent objects having all the appearance of *stars*, which were seen sailing through the field of the telescope. Dr. Wollaston, when this was mentioned to him, said it was thistledown. I do not think it was.

2. In the hay season, some three or four years ago, the day being clear and hot and calm (at least in the immediate neighbourhood of our house), our attention was excited by what at first seemed to be strange-looking *birds* flying; but though presently assured they were not birds, it was by no means clear what they were. They were irregular wispy masses sailing leisurely up and settling down again, apparently over a hay-field on the east of our grounds, and above a quarter or three-eighths of a mile from our house. Some of these were of considerable size, and their general appearance was convex downwards and *taily* upwards. After wondering awhile, I got a telescope and directed it to the flying phenomenon, when it became evident that they were masses of *hay*—some of very considerable size, certainly not less (allowing for the distance) than a yard or two in diameter. They sailed above leisurely, and were very numerous. No doubt wind prevailed at the spot, but there was no roaring noise, nor any sign of a whirlwind, and all about *us* was quite calm. Nobody was at the time at work in that field. None fell on our side of the trees, *above which they rose* perhaps 50 or 100 feet.

P.S. Could Sir R. Inglis's phenomenon have been winged ants? They sometimes appear in astonishing numbers, and might associate like gnats in masses for a dance, and then separate again.

On Meteorological Observations made at Kaaffjord, near Alten, in Western Finmark, and at Christiania in Norway. By JOHN LEE, LL.D., F.R.S., of Hartwell, Bucks.

Dr. Lee stated that he had the honour to present to the Association some meteorological observations, made by Mr. J. H. Grewe, an officer in the service of the Alten Mining Company at Kaaffjord, in Western Finmark, near Alten, under the direction of S. H. Thomas, Esq., the superintendent and able geologist of the Company, for the months of January to September inclusive of 1848; and that they had been made at the same hours of the day and on the same plan as the similar observations which he had the pleasure to present to the Association in former years.

The present observations were accompanied by two new Tables from Mr. Grewe; the first containing *barometrical* means, deduced from the period of eleven years, and arranged in three series:—1st, the monthly means; 2nd the quarterly means; 3rd, the annual means. The second table contained the *thermometrical* means, made simultaneously with those of the barometer, and arranged in similar series. These observations and tables of Mr. Grewe were also accompanied by two tables made by Mr. J. F. Cole, a gentleman now resident in London, but formerly at Alten, and the associate of Mr. Grewe in making some of the earlier observations at Kaaffjord; and Mr. Cole has reduced the observations of Mr. Grewe from the French measures to Fahrenheit's scale; and Dr. Lee produced a letter addressed to him by Mr. Cole, explanatory of the tables.

Dr. Lee also presented to the Association, through the courtesy of J. R. Crowe, Esq., Her Britannic Majesty's Consul-General of Norway, a series of meteorological observations, made during the year 1848 at Christiania. They were stated to be a continuation of observations made at the same hours and on the same plan as others presented on former years.

(Copy.)

London, 8th September 1849.

SIR,—I have had much pleasure in inspecting the Alten Meteorological Observations from January to September 1848, lately transmitted to you by my former colleague, Mr. Grewe.

It appears that from October 1848 the hours of observation have been changed

from 9 A.M., 3 P.M. and 9 P.M. to 7 and 11 A.M., 3, 7 and 10 P.M., in accordance with the suggestion of Professor Hansteen of Christiania.

The present observations are the last of a set of eleven years, and Mr. Grewe has formed a very interesting table of the results of the barometer and thermometer for that time; some of these results I have reduced into English scales, as per tables annexed.

From the table of the results of the observations on the barometer for the eleven years ending 30th September 1848, it will be perceived that the means of the 9 P.M. observations are higher than those of 9 A.M. and 3 P.M., except in November and December, when the 3 P.M. are a trifle the highest.

The observations fall from 9 A.M. to 3 P.M., and then rise to 9 P.M. The monthly means fall from May to June and July, and rise to August, then fall to September, October, November and December, and rise to January, then fall to the lowest mean in February, and rise to March and April, reaching the highest mean in May.

During the eleven years the month of May gives the highest monthly mean, and the month of February the lowest.

The mean of the *monthly means* for May and February (being the highest and lowest) differs from that of the eleven years by only 0·017615 inch; but of the *monthly means*, the one for March comes nearest to that of the eleven years, differing by only 0·00079 inch.

Of the *seasons*, the mean for Spring is the highest and Autumn the lowest. The means fall from Spring to Summer and Autumn, and rise from Autumn to Winter and Spring.

From the tables of the results of the observations on the thermometer for the eleven years ending 30th September 1848, it will be found that the means of the 9 P.M. observations are lower than the 9 A.M. and 3 P.M. *without exception*. Of the *eleven years*, the month of *February* is the *coldest*, and *August* the *warmest*.

The monthly means of the observations rise from March to August, both inclusive, and from September to February, also both inclusive.

The monthly means rise thus :—

From February to March, Fahrenheit.....	5·336
From March to April, Fahrenheit	9·536
From April to May, Fahrenheit	9·396
From May to June, Fahrenheit	8·807
From June to July, Fahrenheit	6·651
From July to August, Fahrenheit	0·254
Total rise.....	39·980

They then fall—

From August to September, Fahrenheit	10·721
From September to October, Fahrenheit	12·522
From October to November, Fahrenheit.....	7·873
From November to December, Fahrenheit	2·855
From December to January, Fahrenheit.....	3·368
From January to February, Fahrenheit	2·641
Total fall.....	39·980

It will be noticed from the foregoing, that the monthly means rise rapidly from March to June, and fall heavily from August to November.

The mean of the monthly means for August and February being the highest and lowest, differs from that of the eleven years by only 1°·594 Fahrenheit.

The nearest monthly mean to that of the eleven years is October, differing from it by only 1°·659 Fahrenheit; this agrees with the result generally noticed by meteorologists.

The means of the seasons :—

Fall from Summer to Autumn	25·785 Fahrenheit
Fall from Autumn to Winter	7·879 "
And rise from Winter to Spring	21·413 "
And rise from Spring to Summer.....	12·251 "

I remain, Sir,

Your obedient humble Servant,
(Signed) JOHN FRANCIS COLE.

Results of Eleven Years' Observations on the Barometer, made at Alten Copper Works, Norway, from October 1837 to September 1848, reduced to English inches.

Months.	9 A.M.		3 P.M.		9 P.M.		Monthly Means.	
	No. of observations.	Mean height for eleven years.	No. of observations.	Mean height for eleven years.	No. of observations.	Mean height for eleven years.	No. of observations.	Mean height for eleven years.
January	339	29.70683	338	29.71329	339	29.71726	1,016	29.71246
February	310	29.64738	310	29.64733	310	29.65770	930	29.65281
March	340	29.75187	339	29.75061	339	29.75986	1,018	29.75411
April	320	29.85671	319	29.84931	319	29.86112	958	29.85573
May	339	29.88730	338	29.89254	339	29.89683	1,016	29.89222
June	328	29.79892	330	29.79860	330	29.81092	980	29.80281
July	332	29.78301	334	29.77155	331	29.78856	997	29.78104
August	326	29.80809	328	29.79864	326	29.81734	980	29.80801
September	323	29.76844	325	29.76372	325	29.78081	973	29.77100
October	341	29.69707	338	29.69262	340	29.70549	1,019	29.69841
November	326	29.66604	327	29.67029	327	29.66667	980	29.66766
December	333	29.65872	331	29.66549	331	29.66329	995	29.66250
Mean of eleven years	3957	29.75254	3957	29.75167	3956	29.76049	11,870	29.75490

Seasons.	9 A.M.		3 P.M.		9 P.M.		Quarterly Means.	
	No. of observations.	Mean height for the season.	No. of observations.	Mean height for the season.	No. of observations.	Mean height for the season.	No. of observations.	Mean height for the season.
Spring	987	29.84766	987	29.84683	988	29.85628	2,962	29.85026
Summer	981	29.78652	987	29.77797	982	29.79557	2,950	29.78667
Autumn	1000	29.67396	996	29.67612	998	29.67848	2,994	29.67620
Winter	989	29.70203	987	29.70573	988	29.71159	2,964	29.70644
Mean of eleven years	3957	29.75254	3957	29.75167	3956	29.76049	11,870	29.75490

Years.	Annual Means.	
	No. of observations during the year.	Mean height for the year.
1837 to 1838	1,045	29.82400
1838 — 1839	1,095	29.74573
1839 — 1840	1,083	29.77490
1840 — 1841	1,064	29.81833
1841 — 1842	1,087	29.74179
1842 — 1843	1,095	28.69510
1843 — 1844	1,098	29.76557
1844 — 1845	1,095	29.78242
1845 — 1846	1,089	29.70372
1846 — 1847	1,068	29.78573
1847 — 1848	1,051	29.73659
Mean of eleven years...	11,870	29.75490

Barometrical Means, deduced from a series of Eleven Years' Observations, made at Kaafjord in West Finmark, latitude $69^{\circ} 57'$, longitude $23^{\circ} 2'$ east of Greenwich.

TABLE I.—Monthly Means.

Months.	9 A.M.		3 P.M.		9 P.M.		Monthly Means.	
	No. of observations.	Barometer.	No. of observations.	Barometer.	No. of observations.	Barometer.	No. of observations.	Barometer.
January	339	754.555	338	754.719	339	754.820	1,016	754.698
February	310	753.045	310	753.196	310	753.307	930	753.183
March	340	755.699	339	755.667	339	755.902	1,018	755.756
April	320	758.362	319	758.174	319	758.474	958	758.337
May	339	759.139	338	759.272	339	759.381	1,016	759.264
June	328	756.894	330	756.886	330	757.199	988	756.993
July	332	756.490	334	756.199	331	756.631	997	756.440
August	326	757.127	328	756.887	326	757.362	980	757.125
September	323	756.120	325	756.000	325	756.434	973	756.185
October	341	754.307	338	754.194	340	754.521	1,019	754.341
November	326	753.519	327	753.627	327	753.535	980	753.560
December	333	753.333	331	753.505	331	753.449	995	753.429
No. of Obs. and } Means of 11 yrs. }	3957	755.716	3957	755.694	3956	755.918	11,870	755.776

TABLE II.—Quarterly Means.

Seasons.	9 A.M.		3 P.M.		9 P.M.		Quarterly Means.	
	No. of observations.	Barometer.	No. of observations.	Barometer.	No. of observations.	Barometer.	No. of observations.	Barometer.
Spring	987	758.132	987	758.111	988	758.351	2,962	758.198
Summer	981	756.579	987	756.362	982	756.809	2,950	756.583
Autumn	1000	753.720	996	753.775	998	753.835	2,994	753.777
Winter	989	754.433	987	754.527	988	754.676	2,964	754.545
No. of Obs. and } Means of 11 yrs. }	3957	755.716	3957	755.694	3956	755.918	11,870	755.776

TABLE III.—Annual Means.

Years.	9 A.M.		3 P.M.		9 P.M.		Annual Means.	
	No. of observations.	Barometer.	No. of observations.	Barometer.	No. of observations.	Barometer.	No. of observations.	Barometer.
1837 to 1838	349	757.309	349	757.526	347	757.758	1,045	757.531
1838 — 1839	365	755.387	365	755.583	365	755.659	1,095	755.543
1839 — 1840	361	756.234	362	756.240	360	756.378	1,083	756.284
1840 — 1841	356	757.389	354	757.320	354	757.452	1,064	757.387
1841 — 1842	362	755.265	363	755.403	362	755.661	1,087	755.443
1842 — 1843	365	754.204	365	754.192	365	754.374	1,095	754.257
1843 — 1844	366	754.269	366	754.123	366	754.414	1,098	754.269
1844 — 1845	365	756.445	365	756.402	365	756.576	1,095	756.475
1845 — 1846	363	754.488	362	754.348	364	754.593	1,089	754.476
1846 — 1847	357	756.587	355	756.402	356	756.689	1,068	756.559
1847 — 1848	348	755.297	351	755.094	352	755.543	1,051	755.311
No. of Obs. and } Means of 11 yrs. }	3957	755.716	3957	755.694	3956	755.918	11,870	755.776

The observations are separately corrected for the temperature of the mercury, capillarity and error of the scale, and reduced to the freezing point, at the level of the sea high-water mark. The series commenced on the 1st of October 1837, and concluded on the 30th of September 1848, both inclusive.

Results of Eleven Years' Observations on the Thermometer, made at Alten Copper Works, Norway, from September 1837 to September 1848, reduced to Fahrenheit's Scale.

Months.	9 A.M.		3 P.M.		9 P.M.		Monthly Means.	
	No. of observations.	Mean height for eleven years.	No. of observations.	Mean height for eleven years.	No. of observations.	Mean height for eleven years.	No. of observations.	Mean height for eleven years.
January	339	18°554	338	19°099	339	17°902	1,016	18°518
February	310	15°498	310	16°795	310	15°341	980	15°877
March	340	20°908	339	23°371	339	19°360	1,018	21°213
April	320	31°912	319	33°012	319	27°322	958	30°749
May	339	41°212	338	42°213	339	37°009	1,016	40°145
June	328	49°366	330	51°359	330	46°132	988	48°952
July	332	56°185	334	58°262	331	52°362	997	55°603
August	326	56°379	328	58°950	326	52°245	980	55°857
September	323	45°185	325	47°307	325	42°919	973	45°136
October	341	32°099	338	33°922	340	31°822	1,019	32°614
November	326	24°850	327	24°985	327	24°390	980	24°741
December	333	21°897	331	21°952	331	21°807	995	21°886
Mean of eleven years	3957	34°504	3957	35°935	3956	32°383	11,870	34°273

Seasons.	9 A.M.		3 P.M.		9 P.M.		Quarterly Means.	
	No. of observations.	Mean height for the season.	No. of observations.	Mean height for the season.	No. of observations.	Mean height for the season.	No. of observations.	Mean height for the season.
Spring	987	40°829	987	42°195	988	36°820	2,962	39°949
Summer	981	52°583	987	54°840	982	49°176	2,950	52°200
Autumn	1000	26°283	996	26°953	998	26°006	2,994	26°415
Winter	989	18°320	987	19°755	988	17°533	2,964	18°536
Mean of eleven years	3957	34°504	3957	35°935	3956	32°383	11,870	34°273

Years.	Annual Means.	
	No. of observations during the year.	Mean height for the year.
From Oct. to Sept.		
1837 — 1838	1,045	32°016
1838 — 1839	1,095	33°028
1839 — 1840	1,083	36°050
1840 — 1841	1,064	33°872
1841 — 1842	1,087	35°013
1842 — 1843	1,095	33°508
1843 — 1844	1,098	35°161
1844 — 1845	1,095	33°841
1845 — 1846	1,089	35°033
1846 — 1847	1,063	35°476
1847 — 1848	1,051	34°018
Mean of eleven years...	11,870	34°273

Thermometrical Means, deduced from a series of Eleven Years' Observations, made at Kaafjord in West Finmark, latitude $69^{\circ} 57'$, longitude $23^{\circ} 2'$ east of Greenwich.

TABLE I.—Monthly Means.

Months.	9 A.M.	3 P.M.	9 P.M.	Monthly Means.
January	— 7.470	— 7.167	— 7.832	— 7.490
February.....	— 9.168	— 8.447	— 9.255	— 8.957
March.....	— 6.162	— 4.794	— 7.022	— 5.993
April	— 0.049	+ 0.562	— 2.599	— 0.695
May	+ 5.118	+ 5.674	+ 2.783	+ 4.525
June	+ 9.648	+ 10.755	+ 7.851	+ 9.418
July	+ 13.436	+ 14.590	+ 11.312	+ 13.113
August	+ 13.544	+ 14.972	+ 11.247	+ 13.254
September	+ 7.325	+ 8.504	+ 6.066	+ 7.298
October	+ 0.055	+ 1.068	— 0.099	+ 0.341
November	— 3.972	— 3.897	— 4.228	— 4.033
December	— 5.613	— 5.582	— 5.663	— 5.619
Means of eleven years..	+ 1.391	+ 2.186	+ 0.213	+ 1.263

TABLE II.—Quarterly Means.

Seasons.	9 A.M.	3 P.M.	9 P.M.	Quarterly Means.
Spring	+ 4.905	+ 5.664	+ 2.678	+ 4.416
Summer	+ 11.435	+ 12.689	+ 9.542	+ 11.222
Autumn	— 3.176	— 2.804	— 3.330	— 3.103
Winter	— 7.600	— 6.803	— 8.037	— 7.480
Means of eleven years..	+ 1.391	+ 2.186	+ 0.213	+ 1.263

TABLE III.—Annual Means.

Years.	9 A.M.	3 P.M.	9 P.M.	Annual Means.
1837 to 1838	+0.808	+0.737	—1.516	+0.009
1838 — 1839	+0.979	+1.522	—0.789	+0.571
1839 — 1840	+2.162	+3.188	+1.400	+2.250
1840 — 1841	+1.059	+1.860	+0.200	+1.040
1841 — 1842	+1.719	+2.518	+0.785	+1.674
1842 — 1843	+0.874	+1.726	—0.086	+0.838
1843 — 1844	+1.709	+2.806	+0.753	+1.756
1844 — 1845	+1.095	+2.021	—0.046	+1.023
1845 — 1846	+1.892	+2.616	+0.547	+1.685
1846 — 1847	+2.054	+3.180	+0.558	+1.931
1847 — 1848	+0.948	+1.876	+0.539	+1.121
Means of eleven years..	+1.391	+2.186	+0.213	+1.263

The thermometrical observations were made simultaneously with those of the barometer. Each observation is separately corrected for the error of scale. The thermometer in the shade stood about three feet above the ground. The scale in Centigrade degrees is graduated on the glass tube.

On the Means of Computing the Quantity of Vapour contained in a Vertical Column of the Atmosphere. By T. HOPKINS.

Mr. Hopkins showed that the quantity of aqueous vapour existing in the atmosphere is computed, by meteorologists of the present day, from the tension of vapour near the surface of the globe in such a way as would be correct if an atmosphere of vapour only existed. But the vapour in our atmosphere is intermixed with and diffused through gases, which gases cool by expansion consequent on the removal of incumbent pressure five times as much as the vapour does. The vapour therefore produced by evaporation at the surface of the globe, as it passes into the higher regions of the atmospheric space, is cooled and condensed, not by its own law of cooling by expansion, but by the cold of the gases; and the result is that a smaller quantity of vapour remains in the atmospheric column with a given temperature and dew-point at the surface than there would be in a pure vapour atmosphere, or than is now said to be indicated by the tension of the vapour found at the surface. That tension he showed was a consequence, not alone of the pressure of an incumbent column of vapour, but also of the resistance which rising vapour encounters from having to penetrate the gases while expanding upwards into the atmospheric space. As soon as elastic vapour is formed the surface of the globe becomes the base on which it rests, and from which it is disposed to expand upwards. But the resistance of the gases prevents free expansion, and preserves a certain amount of density of vapour that would not otherwise be so early attained. The tension of vapour therefore only measures the degree of density that is thus produced, and does not indicate correctly the quantity that exists in the whole atmospheric column. Tables were exhibited by Mr. Hopkins to show the quantities of vapour, expressed in decimal parts of an inch of mercury, that could exist at different heights to the extent of 4000 yards from the surface, in an atmosphere of pure vapour, and also in our mixed atmosphere, each being at the temperature and dew-point of 50° at the surface. And the excess in the quantity of vapour in the former above the latter was stated to show the extent of the error involved in the present mode of estimating the quantity of vapour in a vertical column of the atmosphere with the dew-point named, 50° .

On Meteors. By EDWARD JOSEPH LOWE, F.R.A.S.

See Reports in this volume, page 1.

Notice of a Meteor seen in India on the 19th of last March.
By Admiral Sir C. MALCOLM.

This consisted of selected notices of the meteor from the *Bombay Times* of March and April, and contained several letters detailing the circumstances under which it was seen by the different writers—from which it was inferred to be a mass of over 600 feet in diameter; and the place at which it fell after bursting was ascertained to a high degree of probability: it fortunately was not an inhabited place.

Extract from the Bombay Times, March 16, 1849.

We have letters from Hoshungabad to the 6th inst., which mention that most extraordinary weather had prevailed there during the latter half of February. It had been close, hazy and dry, and a similar state of matters prevailed up to the day above mentioned. On the evening of the 5th the clouds began to collect, the atmosphere having been highly charged with electricity for four days previous,—the electrometer (Cavallo's) readily indicating the amount, and the least friction causing considerable excitation. Pressure and dryness had somewhat increased, and rain was therefore not looked for, but either another earthquake or a thunder-and-dust storm was predicted by the weather-wise. On the 20th of February half a gale had blown throughout the greater part of the day, the mean of the barometer having descended in three days from 29.949 to 29.684! Our correspondent continues: "Prognosticating an earthquake at Hoshungabad is somewhat akin to a pig seeing the wind; but I only hint at such a phenomenon from the consciousness that

there is something very peculiar just now in the atmosphere, and from the fact that the instruments seem as strangely affected as the senses."

We adverted in a former issue to the singular state of the weather at Calcutta, Delhi, along the line of the Jhelum and Chenaub from Rhotas to Mooltan, and near Socotra at Aden, on the 22nd of January; and we now have an account of the appearance at Gibraltar at the same date of nearly the same phænomena which were observed all over the northern part of India. Here, as at Calcutta, Bombay, and Aden, the mercury was remarkable for its elevation; and we have little doubt that were returns obtained from the intermediate points, similar facts would be supplied. Here we have one of the most striking cases of an atmospherical perturbation of simultaneous occurrence we have ever noticed, traceable over one-fourth part of the earth's circumference from east to west, and 20° lat. N. by S. We now have the same striking chain of phænomena from Ceylon, where the heat at Colombo in the last week of January was altogether without precedent in the meteorological annals of the Cinnamon Isle. Hot winds, resembling those of the present year, were last experienced in 1844 [month not given], when they blew from the 16th to the 19th, occasioning much injury to the crops. The waters of the Colombo lake were beginning to dry up, and the canals were nearly useless; many of the wells had run dry. The *Ceylon Times*, to which we are indebted for these facts, assures us that the evaporation amounts to nearly an inch per diem.

On the Results of certain Anemometers. By FOLLETT OSLER.

The author first stated, that, from an aggregate of upwards of 50,000 additional hourly observations, he had been enabled to test the accuracy of the report he brought forward in Glasgow respecting the hourly forces of the wind and their coincidence with the curves of temperature, and that the result was highly satisfactory, being almost precisely similar to that recorded in the report just alluded to, the wind rising with the temperature with great regularity. The curve of temperature for each season corresponded with the curve of force; but from these observations it would appear that the period of mean force in the evening took place about half an hour before that of mean temperature; showing that the motion of the air declines more rapidly than the temperature. The whole of the stations comprise an aggregate of nearly 200,000 hourly observations, all of which were tabulated and reduced. The direction of the wind for each hour of the day, together with its force, was first tabulated, and from this an abstract was obtained, giving the total force and direction for every day. Thus, on the first of January for each year the winds for that day with their forces are recorded; and so on throughout the year. By thus obtaining the exact period of each wind, the coincidences of a series of years were ascertained. In reviewing the Wrottesley observations, which were carried out more fully than the rest, Mr. Osler called attention to the fact of disturbances in the currents of the atmosphere taking place at certain and apparently regular intervals. A comparative calm is followed by considerable disturbances: these calms and movements appear to be periodical. It was possible that observations for a longer term might neutralize these periods, and by shifting their times only leave us with the knowledge that intermittent pulses do occur; but the regularity of some led him to hope that such is not the case, and that a law of periodicity might be traced even in this variable climate.

From six years' records at Wrottesley the average periods of greatest movement in the aerial currents took place towards the end of January, the middle and end of March, the end of April, the early part of June, a short time after the middle of October, about the 20th of November, and the first week in December: the periods of greatest calm occurred about the middle of January, about the 17th of June, and about the 14th of November. There were many other maxima and minima, but Mr. Osler thought it desirable to defer going more into detail respecting them until he had been able further to investigate the subject. On minutely examining the registers of the anemometers two kinds of currents are observed, the one moving very regularly and with great steadiness, the other in larger pulses or waves, causing the vane to oscillate over a considerable arc. One he regarded as the air moving to fill up a void or deficiency; the other, flowing from an excess or from a portion of the

atmosphere, being put in motion and carried on by momentum previously acquired, causing great undulations in its motion, on which occasions the wind appears to have much more force than is really indicated by an instrument. The north winds generally showed less oscillation than those from the south points. While carrying on these observations, Mr. Osler's attention had occasionally been directed to particular storms, and he had applied to them the rotatory theory set forth by Colonel Reid, in the main principles of which he fully agreed; but he considered that a rotating circle would not explain all the changes that occur. He was of opinion that the rotating portion is smaller than has usually been assumed, and that the air approaches this circle or vortex in spiral lines; that sometimes this rotating circle is not in contact with the earth, in which case the lower current will be more in the direction of radial lines; that the air in advance of a storm is not put into such rapid motion in consequence of the movement forward of the storm itself, while, for the same reason, the action in the rear of the storm is increased.

Mr. Osler called the attention of the Section to the great practical importance of endeavouring to ascertain the *exact* course of the wind in rotatory storms, which he considered might be done with the aid of the instruments we now possess. The Americans were beginning to take the subject up in a manner worthy of its importance; and Mr. Osler hoped that while Lieut. Maury and others were at work on the west coast of the Atlantic we should take the east. He recommended that a series of stations for meteorological observations be established, commencing at the Canaries and including Madeira and Gibraltar, the west coast of Spain and Portugal, the Azores, Guernsey, &c. In England, Ireland and Scotland many stations are already established that would no doubt join in contributing observations. By adopting a well-organized and comprehensive system, the main currents on the east of the Atlantic might soon be ascertained with great exactness.

Mr. Osler then exhibited and described his improved integrating anemometer. A plain sheet of paper, about twelve inches wide, and long enough to last some weeks (or months if required), is first rolled on a cylinder whose axis is horizontal, and passing over a second cylinder is received on to a third. Over the central cylinder is placed a registering pencil. The second and central cylinder is made to revolve in proportion to the rate at which the air is passing, by means of rotators exposed to the atmosphere. The direction is obtained by the same means that Mr. Osler adopted with the anemometer at Lloyd's, and which is found to act with great exactness, laying down the direction in a single line free from all oscillations. This is accomplished by means of a fan-sail, similar to that at the back of a windmill, the motion from which is conveyed to a pencil. The paper is ruled as it passes on, by means of pencils indicating the direction. The time is recorded by a clock, which causes a series of small punches marked with the hour to be brought round in succession, one of which receives a blow every hour from the hammer of the clock, and records it on the margin of the paper. Thus a single line gives the direction and quantity of air passing any station, while every hour the clock marks off the time. Mr. Osler pointed out some improvements he had made in his original pressure anemometer, by which he was now enabled to record the force of even light winds at the same time the instrument was strong enough to resist storms. In conclusion, Mr. Osler said that he had long been impressed with the conviction, that if greater attention were paid to a study of the currents of the atmosphere, it would prove to be of more importance in advancing the science of meteorology than anything else that has hitherto been done. It was this conviction that led him to construct his first anemometer in 1836; and though he was not so sanguine as to expect to see meteorology vie with astronomy in its mathematical prophecies, yet he believed that if the subject were taken up on a systematic plan for a short period the results might prove of very great value to science.

On the Temperature of the British Isles, and its influence on the Distribution of Plants. By AUGUSTUS PETERMANN, F.R.G.S.

The author adverted to the climate of Western Europe as being the mildest, comparatively, of all countries in a similar latitude, and showed that a temperate zone, limited by the isothermals of 70° and 30° (Fahr.), extended in

North America from 30° to 51° N. lat.

Asia 30° ... 50° ...

Europe 30° ... 71° ...

The British Isles are situated almost in the centre of this zone. To show the main features of their temperature, the author had constructed on a large map isothermals of the hottest and coldest month (July and January) in the year, based on the observations of about seventy places.

The most striking feature in the *January isothermals* is their general direction from north to south, instead of from west to east, inferring the greatest cold not in the north, but in the east. Between the Shetland Isles and the southern coast of England (except Cornwall and Devon) there is no difference in the winter temperature; but between the eastern coasts of England and the western coasts of Ireland the difference amounts to about 10°; the former being, at an average, 35°, the latter probably 45°. The coldest portion of Britain extends from the Naze to the Firth of Forth, comprising to the west all the Pennine chain; in this district an average temperature of 35° to 36° prevails. On the continent the January temperature becomes lower in going eastward, precisely in the same ratio as in the British Isles, and the isothermal of 28° extends as far west as the meridian of Göttingen and Hanover. In Scandinavia the temperature decreases very suddenly, owing to the snow-clad mountain masses which project in a high rampart on the western coasts. The difference between Bergen and Christiania, two places in about the same latitude, distant from each other 190 miles, amounts to as much as 14°, the former being 35°·0, the latter 20°·8. The author then proceeded to allude to general and local causes, by which these January isothermals are regulated.

The average direction of the isothermals of the hottest month (July) is from S.W. to N.E. The highest summer temperature in the British Isles, indicated by the isothermal of 64°, occurs in the central portion of the south coast of England, the lowest in the N.W. part of Scotland, and the difference appears to be at least 10°; while the difference between the western and eastern coasts is much less. The isothermal of 62° extends to Lincoln, Birmingham, and the southernmost portions of Wales. All Ireland, Wales, northern part of England, and Scotland to the foot of the Highlands, lie between the isothermals of 62° and 60°. North of the Highlands the temperature is very considerably lower, Inverness having only 55°·7. By comparing the British Isles with parts of the continent in the same latitude, we find in that of Dublin, 61°·5, at the Dutch shores 64°, at Hamburg 65°. In the latitude of Inverness (55°·7 temp.), Frederikshaven in Denmark 61°·9, Göteborg in Sweden 63°·2; between this latter place and Inverness the distance is 600 English miles.

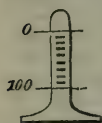
The author then alluded to the influence of temperature on the distribution of plants, the districts of which he had found to be strikingly corroborative with the general correctness of his isothermals (for his botanical observations he was greatly indebted to Mr. H. C. Watson, author of the 'Cybele Britannica,' &c.). There are altogether a good number of plants in Britain which botanists are accustomed to regard as western species, being frequently scattered along the western counties, from Cornwall to Scotland, without passing into the eastern counties, unless at the south or north extremities of Britain. Compared with each other, these western species present much difference in respect to the area, or space of Britain, over which they are distributed respectively. But they correspond in the negative peculiarity of being absent from that part of Britain which extends between the Firth of Forth and the Lincolnshire Wash, and mostly absent from the whole eastern side of the island between the Thames and Murray Firth. This class of plants corresponds in their districts with the January isothermals. Other plants, less impatient of a cold winter, but requiring a higher summer temperature, are found to run parallel with the July isothermals. A great number of species, and the districts where they occur, were named. Among the more important plants being limited by summer isothermals is the vine, the northern limit of which is found to be between the July isothermals of 66° and 67°. In the valley of the Seine, it obtains its highest latitude between Louvier and Andelys in about 49° north lat., but further east, near Berlin, it reaches nearly 52½°, a latitude corresponding with that of Norwich, Birmingham and Limerick.

The author, in concluding his observations, expressed the hope to see this subject further investigated; especially to see the net of meteorological stations over the

British Isles extended and completed,—all Ireland and Wales, as well as the north-western part of Scotland, exhibiting as yet great blanks on an isothermal map.

Contributions to Anemometry—The Therm-anemometer. By JOHN PHILLIPS, F.R.S., Assistant-General Secretary to the British Association.

The author's researches into the force and velocity of wind have been directed to the completion of a method of wind-registration which should be independent of mechanical movements, momentum and friction *. He wished to register the wind by one of the effects of the displacement of its molecules, not the movement of its mass. For this purpose only one method has occurred to him as sufficiently applicable, viz. the evaporation of a liquid. He has experimented on water, saline solutions and alcoholic mixtures, and he finds that with either of these liquids an instrument really indicating the movement of wind, by the registration of the evaporation which the wind causes, is producible. Such an instrument need occupy but a very small space, and will have the desirable quality of being most accurate in those very low velocities of wind which elude entirely Lind's anemometer, and are scarcely sensible by any registering machinery.



It will be remembered, that for the interpretation of the register of evaporation into a register of wind velocity, it was necessary first to correct for the hygrometric state of the air. This being done, the cooling power of wind was found by experiment to be nearly as the square root of its velocity. In this experimental result Professor Phillips was induced to place confidence, because it appeared to represent and flow naturally from what may be thought the true physical action of the moving air.

Having lately occasion to examine extensively and carefully into the amount of air which passes through the ramified passages of collieries, where the currents are sometimes so slow, that machine anemometers, even of a most delicate description, are insensible to the movement of the air—where even the miner's candle affords but a rude guess, and where the situation is such that smoke or the powder flash cannot be appealed to—he was happy to find that the problem was perfectly and easily solved, by noting the cooling power of the current.

For this purpose a registering or integrating anemometer is not required. The currents underground are steady, and require only an anemoscope or indicator of the momentary velocity. Evaporation from the wet-bulb may therefore be abandoned; the common thermometer, with its bulb clear of the frame, will answer the purpose of experiment in every conceivable instance †.

The author stated the general formula, to which he had been conducted by very numerous experiments through a large range of velocities ascertained by other means, thus: $\frac{C}{s^2} - r = v$.

In this formula C and r are constants to be determined for the particular thermometer in use; s being the number of seconds which elapse in cooling through a certain range (5°) from a point a certain number of degrees (10°) above the temperature of the moving air, v the movement of air in a given time (one second).

The explanation is simple. A thermometer-bulb, heated above the temperature of the surrounding air, is cooled by radiation and by convection.

Omitting at present the consideration of the cooling in the thermometer-bulb by radiation (r), the effect of air movement in lowering temperature is proportional to the quantity of air (q) (or number of cooling elements) which passes the bulb, and to the time of its action (s), or to qs , which will be a constant (C) for each instrument. The quantity which passes is proportional to the velocity of the current, and to the time of its action, or $q = vs$. Hence $C = qs = vs^2$, and $v = \frac{C}{s^2}$.

* Reports on Anemometry, 1846, p. 340; 1848, p. 97.

† It appears from Prof. Forbes's Report on Meteorology to the British Association in 1832, that the idea of employing a thermometer for indicating the velocity of wind was entertained by Professor Leslie. It appears never to have been worked out.—(J. P.)

By a great variety of experiments, r , the effect of radiation, is quite unimportant in practice (less than half a foot in a second), except in very low velocities. Its effect in combination appears to be constant for the same instrument, and precisely similar to that of v , so as to be included with it. The formula thus becomes

$$v + r = \frac{C}{s^2} \text{ or } \frac{C}{s^2} - r = v.$$

As already stated, he had made frequent and advantageous use of this cooling property of the air for measuring its velocity in coal-mines. In these situations it is perhaps the only accurate method which can be applied to determine the quantity of air which passes through the extremities of the workings—by the bodies of the workmen engaged in cutting the coal—at particular and critical outlets from the old wastes—in contractions by brattices, scalings, and perforated doors. In one of the most important of these cases, viz. the current of air by the bodies of the workmen in the extremities of the 'intake' system, he found enormous differences in different coal districts; but he forbore to mention them, because statements of this kind, unaccompanied with full explanation, might lead to very erroneous conclusions regarding the relative safety and good management of mines, and prejudice important inquiries now on foot.

Professor Phillips found the therm-anemometer equally available in a great variety of other researches, some results of which he hoped to present hereafter.

On Luminous Meteors. By the Rev. Prof. POWELL, F.R.S. &c.

See Reports in this volume, page 1.

Meteorological Observations made at Huggate, Yorkshire.

By the Rev. T. RANKIN.

On a singular Atmospheric Wave, in February 1849.

By the Rev. T. RANKIN.

On a Phosphoric Phenomenon in a Pond at Huggate, on June 11th, 1849.

By the Rev. T. RANKIN.

This communication described minutely, with all the attendant circumstances of weather, the state of the barometer and thermometers dry and wet, a violent explosion of inflammable gas which took place on the above day, accompanied with smoke, a great noise, and rumbling concussion, such as to alarm several of the inhabitants of the village. The explosion of the gas was propagated along the pond from N.W. to S.E. Into this pond the refuse of the village had been for ages draining, and it was a common receptacle for the dead bodies of various animals.

On Magnetized Brass. By the Rev. T. RANKIN.

This communication was for the purpose of recording the fact that Mr. Rankin had found the northern half of a brazen meridian of a celestial globe to be so strongly magnetic as to deflect a small needle placed near it so much as eight points from its true direction; while the southern part of it seemed to be totally free from magnetism.

On Observations of the Barometer and Thermometer, made during several Ascents in Balloons. By GEORGE RUSH.

The results of five ascents are given by the author, viz. from Vauxhall, May 1837, 4th of September 1838, 10th of September 1838; from Leicester, 27th of June 1849; and Norwich, 4th of September 1849. On the second occasion (the balloon passing into a snow-storm and rapidly descending) the barometer rose to 19 inches, while the thermometer fell to 22°, being 3° below what it had indicated at the great-

est elevation, viz. barometer 14.70 inches, and 24° lower than when the barometer had fallen to 19 inches while ascending, the thermometer then standing at 46°. In the last ascent the aneroid barometer was used; it was found necessary to shake it at pressure 26.50, and it ceased to act at 24.00 pressure. In descending it again began to act at 24.50. The following table gives the principal results:

Temperature of the Upper Regions of the Air corresponding to certain Barometrical Heights, as observed by George Rush, Esq., during five balloon ascents.

Barometer.	Therm. May 1837.	Therm. 4th of Sept. 1838.	Therm. 10th of Sept. 1838.	Therm. 27th of June 1849.	Therm. 4th of Sept. 1849.	Altitude in feet.
30.50	60	74	
30.22						
30.00	60	66.00				
29.90	66		
29.00	60	68.00	
28.00	66.00	
27.00	58	65.51	
26.00	55	64.50	
25.00	52	63.00	
24.00	48	61.00	
23.50	28			6553 for 32° fall.
23.00	56.00	46	61.00	
22.40	54		
22.00	43	54.00	
21.00	53.00	40	52.46	
20.00	36	52.00	
19.00	46.22	35	46.00	13,044 for 20° fall.
18.00	42.00	30			
17.00	39.00	25			
16.00	35.00	20			
15.00	25.00	18			
14.70	25.00	19,303 for 41° fall.
14.30	18.00	18	20,352 for 43° fall.

Note.—It has been determined by M. Gay-Lussac, from observations made by him during a balloon ascent, in which it is stated that at the temperature of 16° Fahr. he attained an altitude of 21,735 feet, the temperature at starting having been 88°, that it therefore decreases at the rate of 1° for 352 feet of elevation.

On Recent Applications of the Wave Principle to the Practical Construction of Steam-Vessels. By J. SCOTT RUSSELL, C.E., F.R.S.

During the last year I have had more than one opportunity of applying the wave principle to the construction of steam-vessels. There is one case, however, in which I have been able to apply it to practice under circumstances of greater complexity and difficulty than have ever occurred to me, and where it has been successful in overcoming difficulties to a greater extent and in a more decided manner than heretofore. The complete success of the principle is no longer to the Members of this Association a matter of doubt, especially where its application is not controlled by peculiar circumstances. But it will be useful to show, that even in cases where the construction is shut up by practical limits and difficulties intractable on ordinary rules, the wave principles afford a safe and useful guide. It is also important to know at every step how far the experiments made on a small scale are borne out by the large, and where the rule is neutralized by the exceptions.

During the last year a very difficult problem was proposed to me by the engineer of a railway company, which required vessels of a very peculiar construction, limited by the conditions of the case in such a manner as to be pronounced by some impracticable. It was this, to build a steam-vessel that should be fast without great length, a good sea-boat without drawing much water, and to carry a great top weight

and yet to swim very light. Besides, this vessel was to be able to go backwards as well as forwards equally well, and though a small boat was to contain great accommodation.

Now it will be easily seen by those accustomed to the wave system, that the problem as thus stated is one to which the wave principle is far from seeming peculiarly applicable. In the first place, it is well known that the wave principle prescribes a different form of the bow for that of the stern, in order to obtain most speed with least cost of power. In the second place, it is known that a high speed requires, on the wave system, a very considerably greater length than was here allowed for the entrance of the vessel, or the lines of the bow. It would therefore seem at first to be a case that would in all probability prove too difficult for the successful application of the wave system. It is on this account mainly that this case seems to me important to the science of naval construction, and to the progress of the wave system, and to the records of the British Association.

There is one more feature in the case which gives it interest. At the same time, the same problem was worked out by another party on another plan of construction, *not* on the wave principle. Another vessel was built under similar conditions, and furnished with engines of the best construction, made by one of the most eminent engineers in England. Both these vessels were built at the same time, and tried under similar circumstances; therefore here was a case in which the practical value of the wave principle has been brought to a test more direct and less questionable than any that was likely to have occurred, and therefore more important to be placed on the records of the British Association.

The first question which will naturally occur to a Member of this Association who recollects this principle, will be this: how could you apply the wave principle in a vessel made to go equally well both ways? The first answer is ready—it is this, that the vessel cannot be made to go as fast as if designed with equal power to go only one way, seeing that in one case she would have a best possible bow and a best possible stern, and in the other case could have neither.

The next point is this, that in both cases, of bow and of stern, it was necessary to have a compromise. Each required to be in turn both bow and stern; this was accomplished in the following manner:—

If there be any point which has more forcibly struck me in the application of the wave principle than another, it is the flexibility of the wave principle—the extent to which it admits of deviations from its strict rules without losing the benefit of its resistance. If it had unluckily been true of this system, that it prescribed an exact mathematical solid in its three dimensions (like Newton's solid of least resistance), to which implicit adherence was imperative, on pain of losing all the benefit proffered, then indeed the system would have been (like Newton's) of little use, from the fact that from causes independent of resistance, ships cannot be solids of revolution, consistent with other qualities. The wave principle, on the contrary, possesses wonderful flexibility, first from the circumstance of its prescribing lines *in one plane* only, and so leaving the other two dimensions in the hands of the practical constructor, so that the sections of the vessel in one plane being given by the system, the sections in two others are at the service of the constructor. This to the accomplished constructor is the greatest possible benefit; to the ignorant constructor it may be considered a great disadvantage, because it affords him no fixed rule in two planes, and so leaves him open to commit a multitude of other errors in points which are not questions of resistance; but to the scientific constructor it gives precisely that latitude which he desires, to leave him free to work out the intentions of the owners and the uses of the ships he may have to build.

There is a second point in the wave system, which is another element of its general usefulness—it partakes of the nature of a mathematical *maximum* or *minimum*. It is the peculiarity of a “maximum and of a minimum,” that deviations on either side of it to a moderate extent occasion deviations of magnitude that are comparatively very small. Thus it is that the wave line being considered as the curve of least resistance, there are near to it an infinite number of approximate curves, which are curves of small resistance, though not of least; and out of these the constructor is free to choose those which shall best accomplish any other object, at the sacrifice of the smallest amount of resistance.

It will readily be seen how these considerations enabled me to obtain in this case the greatest amount of benefit out of the application of the wave system. I had in this case to lay down for both ends of the vessel, that which is best for a bow, and that which is best for a stern, at the given velocity. I had next to place relative values on bow resistance and stern resistance. I had next to single out from between those two lines, one which taken either as bow or stern would deviate least from either, and so have least resistance on a mean of both directions. This therefore the wave principle did; it gave the limits, and gave also the choice of a series of means, all more or less suited to the purpose intended.

I have now shortly to state the practical details by which this process was carried into effect, and the results arrived at in consequence. The engines of the vessel, as well as the vessel, had to be constructed by my partner Mr. Albert Robinson and myself, and we were enabled to adapt the one to the other with greater ease and certainty than in all likelihood we could have done had the engineer been separate from the ship-builder. In one case the engine was considered and made an actual portion of the ship, and the ship of the engine. It will be fair therefore to deduct from the good effects attributed to the wave form of the ship such advantages as we possessed in building both engines and boilers and ship as one whole. Still it is fair to remember, on the other side, that the builders of the engines with which ours had to compete have been celebrated for their efficiency and for the large actual power they have developed, when compared with their nominal power. It should also be remembered that the builders opposed to us had previously built the fastest boats of their district. The only advantages which, consistently with right feeling, we could venture to claim over our competitors, were therefore the use of the wave system, and the having designed both ship, boilers and engines ourselves, and constructed them in our own works as one complete whole.

The practical results obtained are as follows:—

I. Table of Comparative Experiments.

Both vessels were about 150–155 feet long.		
...	...	22–22½ feet beam.
...	...	4 feet draft of water.
...	...	240 tons displacement.
...	...	150 horses' power nominal;

propelled by oscillating cylinders of 48 inches diameter, with the same proportion of stroke to paddle-wheel in both cases; and with only such differences as the engineers and ship-builders in each case considered likely to be most successful in carrying out the execution of their work to the best advantage; the terms prescribed to both builders by the engineer of the proprietors being identical, and with only such latitude as should not form an obstacle to whatever might seem best suited for obtaining greatest efficiency.

Results of Experiments on Velocity with equal Power.

	Wave Vessel.	Competing Vessel.
Speed.....	16.13 miles per hour.	15.03 miles per hour.
Power	20.03 velocity of wheel.	19.09 velocity of wheel.
Loss	4.17 slip of wheel.	4.87 slip of wheel.

These are the results of accurate trials at the measured mile, made both with the tide and against it. It is important to observe the amount of slip, as it serves to show that it was no deficiency of the engine power which caused the difference, both engines having gone at as nearly as possible the same speed. A higher speed might have been taken for the wave-formed vessel, but this is given as that in which the propelling powers were most nearly identical, and therefore the results in speed are most directly comparable.

In order that the statement just given may not lead to false conclusions, it is necessary to state what were those minor differences in vessel and engine which each constructor adopted as tending to greater efficiency. The wave vessel had a flatter floor, five feet longer, and considerably squarer on the midship section; which was done for diminishing the depth of water as wanted for her use. In the other vessel,

the consideration of draft of water was rejected or overlooked, and a finer midship section taken, although with a larger draft of water. In one case also the rudders were considered as part of the length of the vessel and treated accordingly; and in the other case rejected from it. In the engines, although the diameters of the cylinders were identical, the stroke of the wave vessel was somewhat longer than the other, but the diminished effective diameter in the shorter stroke reduced them to nearly the same proportion.

Thus far the experiments given only serve to prove that practically a considerably better result has been obtained by a steam-vessel built on the wave principle than by a competitor built under conditions that are perfectly identical, in so far as the public and the owners are concerned.

But as regards the purely scientific question, I shall add two other experiments with the wave vessel, which furnish data of a more permanent and precise nature—one at a higher, the other at a lower velocity.

Table II.—*Experiments on the Wave Vessel.*

I. Velocity of vessel 15·14 miles an hour.

... wheel 18·17

Slip 3·03

II. Velocity of vessel 16·50 miles an hour.

... wheel 21·20

Slip 4·70

The area of midship section immersed was 89·4 feet.

The surface of vessel immersed was 3080·0 feet.

The area of paddle-floats was 26·8 feet.

The conclusion which I deduce from these last experiments is this, that by means of the wave form one may obtain a form of which the resistance shall be represented by

$$R = \frac{1}{20} A. H. S., \text{ instead of } R = \frac{1}{6} A. H. S.$$

which is the lowest number given in any previous system of construction, A being the area of midship section, H the height due to the velocity of the vessel, and S the weight of a cubic foot of water.

Specimens of Incombustible Cloth. By JAMES LATTO, Dundee.

Specimens of Incombustible Cloth for ladies' and children's dresses, manufactured by Mr. James Latto, Dundee, were exhibited to the Section by Sir David Brewster. This cloth will not catch fire either by a spark or even by contact with a lighted candle, or fire to such an extent as to injure the person who wears a dress made of it. It burns slowly, with a greenish flame, and is speedily extinguished.

Rain or washing deprives the cloth of its difficult combustibility, and it was with the view of directing the attention of chemists to the subject, so as to discover a method of giving the cloth a permanent incombustibility, that Mr. Latto was anxious to have his specimens submitted to the Section.

On Meteorology considered chiefly in relation to Agriculture. By the Rev. Dr. THOMSON.

This was an essay enforcing the importance of meteorological knowledge to those engaged in agricultural pursuits, with numerous suggestions as to courses of observation which it would be desirable to institute.

On Teaching Perspective by Models. By HENRY TWINING.

Mr. Twining exhibited models and demonstrated by figures drawn on glass the importance of having the perspective plane selected in a proper position to the seve-

ral groups to be embraced in the picture, and the distance of that plane properly proportioned to the breadth of the picture.

The President exhibited a Universal Sun-dial, made by Mr. Sharp of Dublin. It consists of a cylinder, set to the day of the month, and then elevated to the latitude. A thin plate of metal in the direction of its axis is then turned by a milled head below it till the shadow is a minimum, when a dial on the top shows the hours by one hand and the minutes by another. It appears that the time can be obtained by this to the precision of about three seconds.

CHEMISTRY.

Inquiries on some Modifications in the Colouring of Glass by Metallic Oxides. By G. BONTEMPS.

IN this communication some important practical points connected with the coloured ornamentation of glass and porcelain were brought forward. In the first place, it was shown that all the colours of the prismatic spectrum might be given to glass by the use of the oxide of iron in varying proportions and by the agency of different degrees of heat; the conclusion of the author being, that all the colours are produced in their natural disposition in proportion as you increase the temperature. Similar phenomena were observed with the oxide of manganese. Manganese is employed to give a pink or purple tint to glass, and also to neutralize the slight green given by iron and carbon to glass in its manufacture. If the glass coloured by manganese remains too long in the melting-pot or the annealing-kiln, the purple tint turns first to a light brownish-red, then to yellow, and afterwards to green. White glass in which a small proportion of manganese has been used is liable to become light yellow by exposure to luminous power. This oxide is also in certain window-glass disposed to turn pink or purple under the action of the sun's rays. M. Bontemps has found that similar changes take place in the annealing-oven. He has determined, by experiments made by him on polyzonal lenses for M. Fresnel, that light is the agent producing the change mentioned; and the author expresses a doubt whether any change in the oxidation of the metal will explain the photogenic effect. A series of chromatic changes of a similar character were observed with the oxides of copper; the colours being in like manner regulated by the heat to which the glass was exposed. It was found that silver, although with less intensity, exhibited the same phenomena; and gold, although usually employed for the purpose of imparting varieties of red, was found by varying degrees of heating at a high temperature and recasting several times, to give a great many tints, varying from blue to pink, red, opaque yellow and green. Charcoal in excess in a mixture of silica-alkaline glass gives a yellow colour, which is not so bright as the yellow from silver; and this yellow colour may be turned to a dark red by a second fire. The author is disposed to refer these chromatic changes to some modifications of the composing particles rather than to any chemical changes in the materials employed.

On an Improvement in the Preparation of Photographic Paper, for the purposes of Automatic Registration; in which a long-continued action is necessary. By C. BROOKE, F.R.S.

The preparation of the paper described previously, may be thus briefly stated. The paper is washed over by a brush with a solution of 12 grs. of bromide of potassium, 8 grs. of iodide of potassium, and 4 grs. of isinglass in one fluid ounce of distilled water, and dried quickly. When about to be used, it is washed over by a brush with a solution of 50 grs. of nitrate of silver to 1 fluid ounce of water, and placed on the cylinder of the registering apparatus, on which it remains in action for twenty-four hours. When removed, the impression is developed by brushing over a warm

solution of gallic acid, containing 20 grs. in the fluid ounce, to which a little strong acetic acid is added, and is then fixed with a solution of hyposulphite of soda in the usual manner. The present improvement consists in rinsing the paper in water after the application of the solution of nitrate of silver, pressing out the superfluous moisture in folds of blotting-paper, and then adding a little more of the solution of nitrate of silver to the surface of the paper; this is most conveniently effected by pouring a small quantity on the paper, and then passing a glass rod or tube lightly over the paper, by which the solution is evenly distributed over the surface, and the contact of organic matter avoided. The increased sensibility and improved cleanliness of the paper consequent on this addition to the process, are presumed to depend on the removal by washing of the nitrate of potash formed by the mutual decomposition of the salts on the surface of the paper.

Researches on the Theory of the principal Phenomena of Photography in the Daguerreotype Process. By A. CLAUDET.

The various questions treated by M. Claudet were the following:—

1. What is the action of light on the sensitive coating?
2. How does the mercurial vapour produce the Daguerreotype image?
3. Which are the particular rays of light that impart to the chemical surface the affinity for mercury?
4. What is the cause of the difference in achromatic lenses between the visual and photogenic foci? Why do they constantly vary?
5. What are the means of measuring the photogenic rays, and of finding the true focus at which they produce the image?

Light produces two different effects on the Daguerreotype plate capable of giving an image. By one the surface is decomposed, and the silver is precipitated as a white powder; this action is very slow. By the other, the parts affected by light receive an affinity for the mercurial vapour, and this metal is deposited in white crystals. This action, which is the cause of the Daguerreotype image, is 3000 times more rapid than that producing the decomposition of the surface. After having examined the phenomena of these two actions, M. Claudet considers that it is impossible to refer them to the same cause. The first is a chemical decomposition of the surface, and the second is a mere new property imparted to the surface to attract the vapour of mercury, which is given by some particular rays and withdrawn by some other rays. The most refrangible rays produce the affinity for mercury, and the least refrangible withdraw it.

M. Claudet afterwards explained the principle of his photophometer, and several improvements he has lately made in that instrument, by which he can compare upon the same plate a series of intensities in a geometrical progression, varying from 1 to 512, and when employing two plates at the same moment, from 1 to 8192; and by another modification of the instrument—by shutting one-half of every hole through which the light has affected the plate, and submitting this half to radiation through red, orange or yellow glasses—he can study the modifications produced on these various intensities of effect, by these coloured or insulated radiations. The experiments to which M. Claudet refers would be too long to enumerate here, and we shall conclude by alluding to the most important point of this paper, which is the question of the difference between the visual and photogenic foci in achromatic lenses, and the constant variations they undergo by the influence of unknown causes, at all events, which he has not been able to ascertain. It is known that several years ago M. Claudet was the first to point out the difference between the two foci, and the necessity for the operator to place exactly the plate at the point where the photogenic focus is produced, in order to have a correct Daguerreotype image. But the new important fact lately observed by M. Claudet refers to the constant variation between the proportionate distance of these two foci. It appears that, according to some causes which M. Claudet has not been yet able to discover, the two foci for the same distance of an object are sometimes coinciding, and sometimes very far, one from the other; and what is most remarkable is, that the difference varies according to some properties of the lenses, in such a manner that when the two foci coincide in one case they may be very much separated in the other.

On the Black Colouring Matter of the Lungs. By Dr. DE VRIJ.

This was a statement of an examination of a peculiar black substance which is often found in the lungs of aged persons. It could not be detected in the lungs of infants; and its nature does not appear to have been yet determined, or the causes which produce it ascertained.

On Artificial Gems. By M. EBELMEN.

This was a note accompanying some specimens of artificial gems prepared by M. Ebelmen under the influences of heat and pressure, as described in his communications to the Academy of Sciences of Paris.

Dr. Percy read a communication from M. Ebelmen, informing the Section that in addition to the specimens of crystallized gems recently furnished, he had now obtained artificially oxide of titanium, niobic acid and tantalic acid by some modifications of his process.

On the Formation of Dolomite. By Professor FORCHHAMMER.

Prof. Forchhammer communicated some observations upon dolomites. He stated that the white chalk of Denmark is covered by a bed only a few feet thick, containing corals of the genera *Caryophyllia* and *Oculina*, and a number of fossils different from those of the white chalk; that this bed, which may be seen over a great part of Denmark always in the same position, the same fossiliferous character, and the same thickness, is enlarged in the hill of Faxøe to a thickness which cannot be much less than 150 feet. Here the Faxøe limestone is covered by a bed of dolomite, which again is covered by a thick bed of limestone, consisting almost entirely of fragments of Bryozoa, and belonging likewise to the chalk formation. The limestone of Faxøe contains about 1 per cent. of carbonate of magnesia, arising from the shells and corals, which always contain it in a small quantity, but which in some instances, as in the *Isis* and some *Serpulæ*, amounts to 6 or 7 per cent. The bryozoan limestone which covers the dolomite contains not more than 1 per cent. of carbonate of magnesia, while the dolomite contains 16 or 17 per cent.

The dolomite occurs generally in round globular masses, very similar to those of Humbledon Hill, and are evidently, like most of the globular masses of limestone, such as confetti di Tivoli and the peastone from Carlshad, the produce of springs; an opinion which is still more confirmed by a number of large vertical tube-like cavities, which pass through the compact limestone, and are completely similar to those described by several English geologists as passing through the chalk, which have been recognized as the natural pipes of springs. Thus the Faxøe dolomite is the produce of springs; but then these springs have deposited stalagmitic limestone wherever they have passed through the crevices of the limestone rock, which, as a more or less thick coating, covers all the fossils. Now this produce of the springs contains only a very small quantity of magnesia, but, besides lime, a great quantity of oxide of iron. It appears thus that if no other reaction takes place than the escape of carbonic acid, the springs do not deposit carbonate of magnesia, but that the dolomite is formed where the carbonic acid springs come in contact with sea water.

The author has made a great number of experiments on the decomposition which takes place when water containing carbonates dissolved by carbonic acid acts upon sea water, and found that always a more or less great quantity of carbonate of magnesia was precipitated with the carbonate of lime. When using water containing only carbonate of lime, the quantity of carbonate of magnesia thrown down at a boiling heat amounted to $12\frac{1}{2}$ per cent., the rest being carbonate of lime. The results of this decomposition vary however very much, and according to conditions not yet well known. So much however may be stated, that the quantity of carbonate of magnesia precipitated increases with the increasing temperature. Water which, besides carbonate of lime, contains carbonate of soda, throws down a much larger quantity of carbonate of magnesia, amounting in one experiment to 27.93 per cent. of the precipitate.

At last the author tried what kind of precipitate some of the most famous mineral springs of Germany would form, if they at the boiling-point acted upon sea water. Thus he obtained from the water of Selters—

Carbonate of lime	86.55
Carbonate of magnesia.....	13.45
	<hr/> 100.00

From the water of Pyrmont—

Carbonate of lime	84.38
Carbonate of magnesia	5.12
Protoxide of iron	10.50
	<hr/> 100.00

The oxide of iron in the experiment was of course precipitated as peroxide of iron, and from that the carbonate was calculated.

From the water of Wildungen—

Carbonate of lime	92.12
Carbonate of magnesia	7.88
	<hr/> 100.00

On a New Method of ascertaining the Quantity of Organic Matter in Water.

By Prof. FORCHHAMMER.

The test which the author applies is hypermanganesiate of potash or soda, which he prepares in this way; he heats the hydrate of potash or soda with chlorate of potash and peroxide of manganese, according to the method of Wöhler. After heating, the salt is thrown into water, and so much diluted muriatic acid is added that it assumes a bluish-red colour, upon which carbonic acid gas is led through, until the colour has become bright red and the manganesiate of potash completely converted into hypermanganesiate. The liquid must be cleared, either by allowing it to deposit all the oxide of manganese, or by filtering it through asbestos. This liquid may be kept for a very long time unaltered in a glass vessel with a glass stopper. The next process is to ascertain the strength of the test, which is done by taking any determined measure of it, mixing it with water and a little alcohol, and then heating it. All the manganese is thrown down, and after being washed and exposed to a strong red heat, it is the compound oxide of manganese, $3\text{Mn} + 4\text{O}$.

This test is now applied in such a way, that, for instance, one pound of the water which is to be tried is mixed with a small quantity of the test and boiled; if the colour has disappeared, another quantity is added, and the liquor again boiled, until, in going on in that way, the red colour of the liquid does not disappear any longer. After that it is allowed to cool, and then the quantity of hypermanganesiate of potash, which has not been decomposed for want of organic matter in the water, is determined by comparing its colour with distilled water, to which have been added very small determined quantities of the test solution. If the quantity of the test which thus is added in excess is subtracted from the whole quantity which has been used, the real quantity of decomposed hypermanganesic acid is determined, and thus also the quantity of organic matter itself. This method is liable to one fault, viz. that the nature of the organic matter may be different, and accordingly require different quantities of the test liquor to be decomposed. But the organic matter which generally occurs in water is approaching almost always to humic acid, and thus the determination of the organic matter is practicable. As to that part of the organic matter in water which contains nitrogen, the author thinks that he has found out a method to determine it by itself; but not having yet finished his experiments on that point, he must leave it out of the question.

Water taken from a greensand spring about twelve miles from Copenhagen, contained so little organic matter that 1 pound only required 6 measures of a test solution, of which 100 measures contained the manganese of 0.526 of the double oxide of manganese, while water taken from a lake which communicates with a peat moss, required for 1 pound 74 measures of the same liquor. Prof. Forchhammer, con-

tinuing for a whole year every week this analysis of the water which is used to provide Copenhagen, observed the following facts :—

1. The quantity of organic matter is greatest in summer.
2. It disappears, for the most part, as soon as the water freezes.
3. Its quantity is diminished by rain.
4. Its quantity is diminished if the water has to run a long way in open channels.

On the Compounds of the Halogens with Phosphorus.

By J. H. GLADSTONE, Ph.D.

It is well known that chlorine, bromine, and iodine will combine directly with phosphorus, yielding compounds containing three atoms of the halogen. Cyanogen does not so combine. If a larger amount of the halogen be employed, compounds are formed containing five atoms. All these substances are neither basic nor acid; they are resolved by water into the hydracids of the halogens, and phosphorous or phosphoric acid. If phosphorus be distilled with chloride, bromide, or iodide of mercury, the ter-compound results. There exists a ter-fluoride (Davy). The ter-cyanide is doubtful.

The penta-compounds of the halogens with phosphorus may easily be reduced to the ter-compounds. The addition of fresh phosphorus will effect this in each instance. Phosphuretted hydrogen has already been observed to reduce the pentachloride; it has the same effect upon the pentabromide. Hydrogen alone has no reducing power upon either of these compounds. Heat alone however will effect the reduction of the pentabromide; if a current of dry air be passed over it at 212° F., so as to remove the free bromine, pure terbromide is obtained. The higher compound of iodine and phosphorus may be similarly decomposed, but not the pentachloride.

The force of affinity for phosphorus is in the order—chlorine, bromine, iodine. The ter-compounds are not directly acted upon by oxygen or sulphur, but suffer double decomposition by water or hydrosulphuric acid. The comparative feebleness with which the two additional atoms of the penta-compounds are combined, is also evident from the action of certain non-metallic elements; thus, iodine reduces the pentabromide of phosphorus. The moderated action of water upon the pentachloride forms an oxychloride of phosphorus (Wurtz), and there exists an oxybromide of phosphorus, $PBr_3 O_2$, exactly analogous. No compound, similar to the sulphochloride of phosphorus of Serullas, is formed by the action of hydrosulphuric acid on the pentabromide, but a liquid, the analysis of which appears to suggest the composition $3PBr_3 + PS_3$. No such compounds exist in the iodine series.

The increased stability, which the substitution of two atoms of oxygen, or sulphur, for two atoms of the halogen, imparts to the remaining three atoms, is manifest from—1, the non-action of hydrosulphuric acid; 2nd, the fact that metals, even potassium, are not attacked by them; 3rd, the non-action of phosphorus.

Hydrochloric acid produces no partial double decomposition with the pentabromide or pentiodide. Nor does any halogen combine directly with the ter-compound of another with phosphorus. If bromine and iodine be presented simultaneously to the terchloride, pentabromide of phosphorus is formed. A colourless crystalline body exists, belonging to the bromine series, never obtained in quantity, possibly isomeric with the oxybromide.

Sulphur appears to combine directly with pentachloride of phosphorus when they are fused together; a crystalline body, and a straw-coloured liquid result. The analysis of these compounds is attended with peculiar difficulty, nor had the author sufficient time to investigate all the anomalies of their decomposition. The straw-coloured liquid however would appear to be $PCl_5 S_4$. The provisional name of Sulphurets of the Pentachloride of Phosphorus is given.

On a continued spontaneous Evolution of Gas at the Village of Charlemont, Staffordshire. By SAMUEL HOWARD.

In a field by the side of a lane, near the village of Charlemont in Staffordshire, certain patches of ground had been noticed, which, without any apparent cause, were destitute of vegetation. They excited little attention, as they were supposed to be

what are commonly called fairy rings, and it was not till the summer of 1846 that their true character was discovered.

The person who first paid particular attention to the cause of these barren spots, was the tenant of a neighbouring cottage (at which there is a cold bath, noted in the vicinity for its sanative properties). From certain circumstances he was led to believe that something permeated the earth in those spots; and having dug a hole, he inserted a gas-pipe, and on applying a light to the mouth of the pipe, he found to his great surprise that a large flame issued from it. It was not long before he conceived the idea of applying it to domestic purposes, and in pursuing his experiments he found that it was not necessary to convey it from the place where it was first discovered, at a distance of about 150 yards from his house, as on driving a pipe some inches into the ground under the floor of the cottage, he procured a continuous flow of gas.

There are at the present time seven burners in the cottage, which enable the owners to dispense with fire and candles. The next cottage is also supplied with two. It appears to make no difference to the supply of gas if allowed to burn for weeks together, and the flame is always of the same colour. In windy weather the flame is generally unsteady; when there is a blast of wind outside the flames of gas rise several inches, but as each blast dies away they return to their original size. The escape of gas is larger in wet weather than in dry; but whether the gas is produced near the surface or otherwise has not yet been satisfactorily ascertained. The place where it issues from the earth is quite a mile from any coal-pit, and is outside the eastern edge of the Staffordshire coal basin.

The gas, as analysed by myself from a portion of it (procured for me by Mr. S. Lloyd, jun. of Wednesbury, about three miles from the place), was composed principally of light carburetted hydrogen. In 1000 volumes of the gas, as it rises, I procured 996 vols. of light carburetted hydrogen, 3 of carbonic acid, and 1 of aqueous vapour and nitrogen. Its specific gravity is 0.56126. Its composition is somewhat different from the gas known as marsh gas, and from that which collects in the old workings of mines, as it contains less carbonic acid, and less nitrogen; the proportion in marsh gas of the former being $\frac{1}{20}$ and of the latter $\frac{1}{15}$ to $\frac{1}{20}$, whereas in this gas the proportions are only $\frac{1}{1000}$ and $\frac{1}{1000}$.

It burns with a pale bluish-white flame, emitting considerable light and heat. Mixed with atmospheric air or oxygen, it explodes with considerable violence on contact with flame, or with the electric spark. As it issues from the pipe it has a moist or slightly musty smell, as of sticks partially decomposed, but after keeping for some time in stopped glass jars this is lost, and it becomes perfectly inodorous. When inhaled in large quantities it produces the same effects as hydrogen gas, but it does not appear to exert any evil influence on the health of the inhabitants of the cottage, when diluted with a large portion of atmospheric air.

On Copper containing Phosphorus, with Details of Experiments on the Corrosive Action of Sea-water on some Varieties of Copper. By JOHN PERCY, M.D., F.R.S.

Upon analysing a specimen of copper, to which when in a state of fusion some phosphorus had been added, it was found that it contained a considerable quantity of phosphorus, and also a large portion of iron derived from an iron rod employed in stirring the mixture at each addition of the phosphorus. The copper employed was of the "best selected"—it appeared to be harder than copper treated with arsenic. The details of the analysis of 116.76 grains were given, the result of which was—

Phosphorus.....	0.93
Iron	1.99
A second analysis gave—	
Copper	95.72
Iron	2.41
Phosphorus.....	2.41

100.54

It has long been stated that a very small quantity of phosphorus renders copper extremely hard, and adapts it for cutting instruments, but such an alloy as that formed

by Dr. Percy has not previously been formed. It is a remarkable fact, that the presence of so large a quantity of phosphorus and iron should so little affect the tenacity and malleability of the copper. The effect also of phosphorus in causing soundness in the casting of copper is interesting, and may be of practical importance. Some experiments were next described, made by Capt. James of Portsmouth, bearing on the æconomic value of the alloy of phosphorus and copper. By the experiments made by Capt. James on the corrosive action of sea-water, it would appear that this compound was much less affected than most other specimens of copper tried. The results derived from exposing measured pieces of copper to the action of sea-water for nine months were as follows:—

	grains.
Electrotype copper, loss per square inch	1·4
Selected copper	1·1
Copper containing phosphorus	·0
Copper from the "Frolic"	1·12
Deckyard copper, No. 1	1·66
Ditto No. 2	3·00
Ditto No. 3	2·48
Ditto No. 4	2·33
Muntz's metal	0·95

The results appear to be of sufficient importance to excite attention to the fact, and to elicit further inquiry, especially when it is remembered how important and æconomic a desideratum it is to the Admiralty to diminish or prevent the corrosive effect of sea-water upon copper.

On the Decomposition and partial Solution of Minerals, Rocks, &c. by pure Water and Water charged with Carbonic Acid. By Prof. W. B. ROGERS and Prof. R. E. ROGERS, of the University of Virginia.

In opening this communication, Prof. W. B. Rogers adverted to its important bearings upon the chemistry of geology, and the theories of the formation of soils and of the nutrition of plants. He referred to the comparatively isolated experiments of Struvé, Forchhammer and others, as being of too restricted a scope to furnish a basis for reasoning *generally* on the disintegration of rocks, the formation of chalcedonic, zeolitic and other minerals by solution, and the conveyance of inorganic materials into the structure of plants. It therefore becomes a question of importance, whether water pure, or charged with carbonic acid, possesses that general decomposing and dissolving power which some chemists have vaguely and without sufficient evidence ascribed to it, or whether this action applies only to the few materials hitherto tried, and which all contain an alkali.

The experiments of the Professors Rogers were of two kinds; first, by an extemporaneous method *with the tache*; and secondly, by *prolonged digestion* at the ordinary temperature. In the former, a small quantity of the mineral in very fine powder is digested for a few moments on a small filter of purified paper, and a single clear drop of the liquid received on a platinum slip is dried and examined by appropriate tests before and after ignition. In the second process a quantity of the finely-powdered mineral is placed with the liquid in a green glass bottle and agitated from time to time for a prescribed period. The liquid separated by filtration is evaporated to dryness in a platinum capsule. The residuum is then critically examined, and, if in sufficient amount, is submitted to quantitative analysis.

In both processes two parallel experiments were made, the one with pure aerated water, the other with water charged to saturation at 60° with carbonic acid. In the second process, correction was made for the alkali, lime, &c. dissolved from the containing glass, by making separate experiments in similar vessels without the mineral powders.

1. When the substance is very minutely powdered before mingling it with the liquid, even the first drops that pass the filter will commonly give a *tache* containing some of the alkali or alkaline earth that has been dissolved. In this way proof of the action of the carbonated water may generally be obtained in a few minutes after adding it to the powder. In the case of pure water the action is feebler and requires

a longer time, but with nearly all the substances enumerated it is distinct, and with some of them quite intense.

2. By an independent series of experiments to determine the effects of heat, which were made upon the *taches* of potassa and soda and their carbonates, and upon those of carbonate of lime and magnesia, as well as upon considerable quantities of these substances successively exposed in a crucible to the heat of the table blowpipe, it was found that the order of volatility was as follows:—potassa, soda, magnesia, lime. The *tache* of potassa disappeared almost at once, that of soda lingered some time, that of magnesia wasted more slowly, while that of lime remained with little alteration for a long time.

Before heat was applied the *tache* of the alkalies or their carbonates would of course be strongly alkaline. That of the carbonate of magnesia also presented a decided and sometimes strong reaction with the test-paper, while that of carbonate of lime gave a merely appreciable effect. But on raising the *tache* to a red heat, the carbonate of lime, by escape of carbonic acid, would acquire intense alkalinity, the reaction of the magnesia *tache* would be but little altered, and that of the alkaline *taches* would be almost or entirely destroyed.

As examples of this *distinctive* testing and of the mode of proceeding in these *tache* experiments, Professors Rogers gave some details, extracted from the large mass of unpublished results, and called attention particularly to the contrasting phenomena in the cases of Leucite, Olivine and Epidote; the first characterized by potassa, the second by magnesia, and the last by lime.

Thus in the case of *Leucite*, the water *tache* and carbonic acid water *tache* were both alkaline, the latter very strongly so. But even gentle ignition for a few seconds, or strong ignition for a moment, was found *entirely* to dissipate the alkali.

In the case of *Olivine*, the water *tache* was decidedly alkaline, and that from carbonic acid water greatly more so. Ignition produced for the first second or two but little change, but its continuance caused a gradual diminution of the alkaline reaction, which at the end of ten seconds was reduced to about *one-twelfth* of what it was at first.

With *Epidote* the *tache* presented an extremely feeble reaction before heating. Ignited for a moment, the alkalinity was intense, and after ten seconds of ignition but *little abatement* of the alkaline reaction was discerned.

3. Referring to the second method of experimenting used by the Professors Rogers, viz. that of *prolonged digestion* in water or carbonic acid water, Profs. Rogers exhibited results obtained with hornblende, epidote, chlorite, mesotype, &c., showing that the amount of solid matter dissolved by the carbonated water in many of these cases is quite sufficient for a *qualitative analysis*, even when the digestion has only been continued for forty-eight hours. When further prolonged, they have procured from the liquid a quantity of lime, magnesia, oxide of iron, alumina, silica and alkali, the dissolved ingredients of these minerals severally amounting sometimes to nearly one per cent. of the whole mass.

4. In connection with the preceding investigations, the Professors Rogers were led to an examination of the *comparative solubility of carbonate of lime and carbonate of magnesia in carbonated water*. In the standard chemical and geological works the carbonate of lime is stated to be the more soluble, and on this supposed fact is founded a common theory of the origin of the large quantities of carbonate of magnesia in the magnesian limestones. It was conceived that in a mixed limestone containing both the carbonates, the *relative amount* of carbonate of magnesia would be augmented through the more rapid removal of the carbonate of lime by the percolating waters, and that thus the mass would approach more and more to the composition of a dolomite.

The experiments of the Professors Rogers demonstrate that in water impregnated with carbonic acid, carbonate of magnesia is much more soluble than carbonate of lime. Thus, by allowing the slightly-carbonated water to filter through a mass of magnesian limestone in fine powder, and collecting the clear liquid, analysis detected a much larger proportion of carbonate of magnesia in the solution, in comparison with the carbonate of lime, than corresponded with the amount of these substances relatively in the powdered rock. Again, by agitating briskly a quantity of the powder with the carbonated water in a glass vessel and then separating the liquid by filtration, it was

found that a larger relative amount of the carbonate of magnesia had been taken up by the solvent than of carbonate of lime.

From these experiments the Professors Rogers infer that the infiltrating rain-water, with its slight charge of carbonic acid, in passing through or between strata of magnesian limestone, will remove the carbonate of magnesia more rapidly than the carbonate of lime, and that thus the rock will gradually become relatively less magnesian, instead of being made to approach the condition of a dolomite, as is commonly maintained.

Professors Rogers called attention to the fact, that the stalactites in caverns of magnesian limestone contain only minute quantities of carbonate of magnesia. An examination of those in Weyer's cave in Virginia had proved that while the milky white opaque stalactites contain a small but measurable amount, the sparry and more transparent kinds are almost destitute of a trace of this ingredient. It is evident that in such cases the carbonate of magnesia is carried off by the liquid below, and that such is the case seems to be confirmed by the fact of the large amount of carbonate of magnesia found in the springs in the immediate neighbourhood of the cave just named.

5. A fact of much interest noticed in these experiments is the comparative readiness with which the magnesian and calcareo-magnesian silicates yield to the decomposing and dissolving action of carbonated water and even simple water. This explains the rapid decomposition of most rocks composed of hornblende, epidote, &c., without calling in the agency of an alkali, and it enables us to trace the simple process by which plants are furnished with the lime and magnesia they require from soils containing these silicates, without our having recourse to any mysterious decomposing power of the roots of the growing vegetable.

6. In their *tache* experiments, the Professors Rogers ascertained that the powder of anthracite, bituminous coal and lignite all yielded a discernible amount of alkali to the carbonated water, while the ashes of these materials, similarly treated, gave no alkaline trace on the test-paper. This they think is at once explained by the high temperature at which the ash is formed, which by experiments already noticed is quite sufficient to dissipate any portion of alkali or carbonate originally present in the material.

On the Allotropic Condition of Phosphorus. By Prof. SCHROETTER of Vienna.

This communication being already before the world in the 'Annuaire de Chimie' of Millon and Reiset, it is unnecessary to do more than briefly state the facts, which Prof. Schroetter illustrated by experiment. When phosphorus is exposed to light or heat, it is found that a peculiar change of colour takes place, and that although it undergoes no chemical change, a very remarkable physical difference is found to have ensued. The ordinary yellow phosphorus is highly inflammable. The allotropic red phosphorus was not ignited by friction, nor by those agents which acted energetically upon the common variety.

On the combined Use of the Basic Acetates of Lead and Sulphurous Acid in the Colonial Manufacture and the Refining of Sugar. By Dr. SCOFFERN.

Dr. Scoffern, after a few preliminary remarks on the anomalies which beset the colonial manufacture of sugar, stated the actual amount of pure white and crystallizable sugar existing in the sugar-cane juice to be from 17 to 23 per cent., and the amount of juice contained in the cane to be about 90 per cent. Of this amount only 60 per cent. on an average is extracted, and of this quantity only one-third part of its sugar is obtained, in a dark impure condition, instead of white and pure, as it might be extracted. The operation at present generally followed in the colonial production of sugar involved the use of lime, an agent which, although beneficial in separating certain impurities and decomposing others, effects both these agencies at the expense of two-thirds of the original sugar.

Various plans had been followed to avoid the use of lime; alumina in its hydrated condition had been employed, but with inconsiderable success. As a purifying agent the basic acetate of lead was known to be most potent, but could not be generally employed, owing to the existence of no efficient means of separating any excess of

that agent which might remain. Dr. Scoffern effects this separation by means of sulphurous acid forced by mechanical means into the sugar solutions. The process had been used for more than twelve months in one of the large British refineries, and a lump of sugar prepared by means of the operation was exhibited.

The advantages presented by this operation were thus summed up:—

1. As applied to cane-juice, and other natural juices containing sugar, it enables the whole of the latter to be extracted, instead of one-third, as is now the case, and in the condition of perfect whiteness, if desired, without the employment of animal charcoal. Owing to the complete separation of impurities, the juice throws up no scum when boiled, and therefore involves no labour of skimming. Finally, the process of curing is effected in less than one-third of the present time; and the sugar being in all cases pure and dry, no loss in weight occurs during the voyage home.

2. As applied to the refinery operation, it enables the manufacturer to work upon staples of such impurity that he could not use them on the old process. It yields from these staples a produce equal in quality to the best refined sugars produced heretofore, in larger quantity and in less time. It banishes the operation of scum pressing, the employment of blood and lime. Finally, its cost is even less than that of the present refinery process.

On the Composition of the Ash of Armeria maritima, grown in different Localities, and Remarks on the Geographical Distribution of that Plant, and the Presence of Fluorine in Plants. By Dr. A. VÆLCKER.

The presence of iodine in plants growing near the sea, and the absence of that element in the same species of plants growing in inland situations, have been noticed some years ago by Dr. Dickie of Aberdeen, who likewise found that in the former soda was more abundant, and potash prevailed in the latter. The author found Dr. Dickie's observations confirmed by his own, and no qualitative analyses of the sea-pink (*Armeria maritima*) having been made, he analysed the ashes of specimens from three different localities, and obtained the following results (the carbonic acid and sand found by actual experiment having been deducted, the result calculated for 100):—

	No. I.	No. II.	No. III.
Potash	8·86	8·85	13·81
Soda	4·47
Chloride of potassium	8·22	26·65
Chloride of sodium	24·03	18·44
Lime	13·50	14·44	9·12
Magnesia	10·98	11·95	4·28
Oxide of iron	7·92	6·83	6·62
Alumina	1·97
Phosphoric acid	5·77	11·75	21·07
Sulphuric acid	7·92	8·68	7·33
Silicic acid	14·58	10·84	11·12
Iodine	Traces.
Fluorine	Traces.	Traces.	Traces.
	100·00	100·00	100·00

No. I. was grown near the sea-shore, and washed by the sea-spray at high water.

No. II. was grown on an elevated granitic rock opposite the former locality.

No. III. in Mr. Lawson's nursery near Edinburgh.

Several observations are suggested by the inspection of the above results:—

1. The proportion of alkaline chlorides, as well as that of silica, in all three ashes is considerable.

2. The quantity of soda is more abundant in the ash of specimens grown near the sea-shore, whilst potash prevails in those grown on the rock.

3. Soda is entirely replaced in the ash of *Armeria maritima* grown in the nursery.

4. The larger quantity of phosphoric acid and potash in the ash of specimens grown in the nursery, viewed in connection with the greater vigour and the somewhat changed natural character of the cultivated plant, appears to exercise a great influence on the natural character of *Armeria maritima*.

5. Traces of fluorine, hitherto found in only few plants, were distinctly detected in all three ashes; iodine only in specimens grown near the sea shore.

The author then adverted to the geographical distribution of the sea-pink in Germany, and represented the above analyses as well-calculated to throw light on the causes which contribute to chain some plants to a particular well-defined geognostic formation, by showing that a soil deficient in soluble silica and alkaline chlorides, of which the sea-pink requires a considerable quantity, is unable to sustain the life of that plant. According to Schleiden, the sea-pink, found everywhere upon the arid sand-dunes of the northern coasts of England, is universally distributed over the sandy plains of northern Germany. In middle and southern Germany it is found only in a few places, and these are distinguished by their arid, sandy character; and curiously enough, we find that the *Armeria maritima* disdains the richest soils in its range of geographical distribution. Thus we find in northern Germany the granite, clay-slate and gypsum of the Hartz mountains, and the porphyry and muschelkalk of Thuringia, setting a limit to the *Armeria maritima*, and we meet with it only until we arrive at the Keuper sand plains in the neighbourhood of Nüremberg. In southern Germany it is found extending through the Palatinate, but neither on the Suabian Alps nor the whole alpine region is it found, and it appears at last again on the sandy plains of northern Italy. The fact that the sea-pink is not found in every sandy soil in Germany, suggests the idea that those inland localities where it occurs have been perhaps the bottoms of ancient lakes, and that the soil in these places will contain much salt. In England and Scotland the sea-pink is found universally on the sea-coasts, but with a few exceptions, we do not meet with it in inland situations. A remarkable exception of this general rule of its geographical distribution in England is offered by the appearance of *Armeria maritima* on the summits of several mountains of the Scottish highlands. How does it happen that it does not occur in the lowlands and localities much nearer the sea? The author regretted to have been unable to procure the material for an analysis, which might probably have assisted him in throwing light on the subject; but expressed the hope to be enabled to examine the ash of specimens from the Highlands in the course of the current year, specimens having been promised to him by Prof. Balfour of Edinburgh. In the meantime he communicated an analysis of dried specimens which he obtained from the herbarium of the Botanical Society of Edinburgh, but for obvious reasons he does not put much confidence in the accuracy of these analytical results. The analysis however indicated likewise a considerable amount of alkaline chlorides in the ash of *Armeria maritima* from the Scottish Highlands. *Armeria maritima* is not the only marine plant which presents this peculiarity; several others, for instance *Plantago maritima*, are found under similar circumstances. Having had no opportunity of examining the localities in the Highlands where these plants occur, the author declined to enter on the theory of this peculiar occurrence, further than to ascribe an important share to the salt, which in the spray of the sea is often carried to considerable heights into the air, and which, it is not unreasonable to suppose, has been deposited again by the rain, particularly in those places which are exposed to regular sea-winds, in such quantities as to answer to the requirements of the sea-pink and other marine plants. He consequently recommended naturalists interested in the subject to ascertain whether those localities in Highland mountains, where these marine plants occur, are exposed to frequent sea-winds or not, and to pay general attention to the meteorological conditions of these places.

In conclusion, the author stated that distinct traces of fluorine had been detected in the three different ashes of *Armeria maritima*, and likewise in the ash of *Cochlearia officinalis*. In the ashes of Dutch Kanaster tobacco no fluorine could be detected, but as tobacco leaves are soaked in water when prepared for Kanaster, it may be that the trace of fluoride of calcium, if present, has been dissolved out by the water, fluoride of calcium having been shown to be soluble in water, to some extent, by Dr. G. Wilson of Edinburgh.

The simultaneous presence of silica in the ashes of most plants renders the detection of fluorine rather difficult, because the methods hitherto known for tracing the presence of fluorine in siliceous mixtures are impracticable, in all cases in which we have to deal with traces of fluorine and large quantities of silica. By following a plan recommended by Dr. G. Wilson, the author was enabled to prove distinctly the

presence of fluorine in the above plants, and he is confident that other chemists, following the same direction, will find it in other plants in which it is likely to occur.

On a Form of Galvanic Battery. By W. H. WALENN.

The present form of battery has been the result of an attempt to combine the principles of the batteries now in use, and to avoid some of their present inconveniences.

Its metallic elements are,—highly carbonized cast iron as a negative plate, and zinc, prepared in a way to be described afterwards, as a positive plate.

The solution is formed by dissolving some of the cast-iron plates, intended to form the negative plates, in one part by measure of oil of vitriol to eight of water, and when there is no free acid in the liquor, adding one-eighth of oil of vitriol.

In the last battery made (one of 6-inch square plates) the zinc plates were prepared by dipping them in dilute sulphuric acid, to cleanse them, washing them well in water, then dipping them in a solution of acetate of lead, and drying the laminar deposit thus obtained over a charcoal fire; mercury, with a little dilute sulphuric acid being then rubbed over the plate, unites with the lead, and this amalgam with the zinc; the excess of mercury is then driven off by second heating over a charcoal fire, and the plate is prepared.

In the form which I have employed, the plates are fixed in a skeleton frame of wood, one-sixteenth of an inch apart, alternately iron and zinc, with glass plates between every metallicity connected pair; the frame with its plates is then placed in a trough (of glass in this instance) containing the solution as made above.

The quantity of electricity passing has been tested with two separate galvanometers, and found to be half that evolved from a Maynooth battery, with plates of the same area, in a given time: this experiment has been repeatedly tried when the battery has been just put to work, and when it has been at work with a galvanometer in the circuit a whole day.

One galvanometer was not very delicate, either in the mounting of the needle or the thinness and number of convolutions of its wire, being designed rather for the measure of large quantities of electricity, than to test the existence of a small amount.

The other was extremely delicate in the mounting of the needle, and therefore could be depended on for its registrations; the current from a single cell battery, having a positive plate of 16 square inches active surface, and two negative plates of the same active surface each, passing through a wire one-sixteenth of an inch diameter, and $1\frac{3}{4}$ inch from the needle, immediately beneath it, deflected the needle more than 30° .

In estimating the quantity of electricity evolved from different batteries, Barlow's theorem was used; viz. that the quantity of electricity passing through a given galvanometer is directly proportional to the tangent of the angle of deflection.

It was observed, in testing the above single cell battery, that in every instance when the battery contact was broken, a small bright spark was visible in daylight.

It has been remarked that the longer the same solution has been used in an active battery, the longer will the addition of a given quantity of sulphuric acid keep the flowing current of electricity constant; also that the battery is much more energetic if it be left out of action, for a time equal to that during which it has been in action, immersed in water; it is also necessary that a considerable volume of solution should surround the plates.

This battery is clearly a combination of the principles of the batteries known as Daniell's, Smee's, Van Melsen's, Chevalier Bunsen's, Sturgeon's, the Maynooth, Schönbein's Inactive and Active Wrought Iron Batteries, and Robert's.

The following advantages peculiar to these batteries, follow from what has been said above:—

1. Great strength of current both in intensity and quantity.
2. Constancy of action.
3. Protosulphate of iron in pure crystals, and pure carbon in fine powder as a saleable residuum, also a sulphate of zinc.
4. Very great reduction in the current expense of batteries as well as their first cost, porous tubes not being used.

5. The plates may be placed at the sixteenth of an inch, and even less, apart: an enormous acting surface may thus be obtained in a very small space, and an additional strength of galvanic current, owing to the nearness of the plates.

6. There is no danger of the boiling of the solution.

7. The plates once arranged in a suitable framing would not require to be disturbed for a very considerable period.

Since the above was written, further experiments have been made, in order to simplify the method of preparing the zinc plates, and the following is the method which appears the best:

After the plates are cleaned with emery, immersion in dilute sulphuric acid, and then in water, they are dipped into a mixture of about equal parts by measure of saturated solutions of chloride of mercury (corrosive sublimate) and acetate of lead; they are then rubbed with a cloth and washed, and are ready for use.

The superiority of this method of preparing the plates consists in the fact, that local action is entirely prevented, and they only require one preparation until they are quite dissolved; they are not so liable to break as common amalgamated plates are, and are therefore able to be used as long as any metal remains.

They are also more highly positive than common amalgamated zinc plates.

On Motions exhibited by Metals under the Influence of Magnetic and Diamagnetic Forces. By W. SYKES WARD.

In the course of a series of experiments in relation to diamagnetism, I observed that the nature of the action upon many metals varied with the intensity of the magnetic force; and I found that such effects were in accordance with the observations of Prof. Plucker, "that the diamagnetic force increases more rapidly than the magnetic in relation to the power of the exciting magnet." I took considerable care in procuring specimens of pure silver, copper, lead, tin and zinc, and found that these assumed the magnetic or diamagnetic state according to the power of the magnet employed. I found a magnet of very moderate size and power sufficient if the polar pieces were brought near to each other, and the metals, the subject of experiment, were in small discs and delicately suspended.

My attention being particularly directed to the phænomena which Dr. Faraday terms revulsion, I observed that the direction of the revulsive motions changed when the magnetic or diamagnetic state of the metal was changed.

When the polar pieces were adjusted within one quarter of an inch apart, and the disc of metal so suspended that one-half was without, and the other half between the polar pieces, another series of phænomena presented themselves. On developing the magnetic force, the disc moves as a pendulum, with a tendency to pass outwards from between the polar pieces; on breaking contact, the disc moved in the reverse direction, tending to pass within the polar pieces. Such motions are remarkable, in that the direction of them is alike in all metals. Such motions appear to result from electrical currents rather than from magnetic or diamagnetic forces; for on substituting for the disc of metal a flat spiral of insulated wire, they were not produced; but on using a similar spiral, but of which the ends of wire were in good contact, the like phænomena were observed as with a disc.

On a Theory of Induced Electric Currents, suggested by Diamagnetic Phænomena. By W. SYKES WARD.

The phænomena mentioned in the foregoing paper involve many points which cannot be easily accounted for according to the received theories of magnetism. Ampère's theory may account for magnetic or diamagnetic phænomena taken separately, but not easily for the changes of condition which take place in the same metal, still less for the changes in the direction of the revulsive motions, particularly those which follow the sluggish condition of the metal under the influence of that amount of force by which the magnetism or diamagnetism are nearly balanced.

It also appears that the induced or secondary electric current may be accounted for on the hypothesis that the current in the primary conductor effects a molecular

disturbance in the parallel or secondary conductor (such disturbance being in the nature of a magnetic affection), and that such disturbance correlatively induces the secondary current, both when it is produced and when it ceases. This hypothesis is also in accordance with the fact that this induced current is only transient, and also appears the best explanation why the induced is not of equal duration with the inducing current.

On the comparative Cost of working various Voltaic Arrangements.

By W. SYKES WARD.

The author stated that a series of calculations, founded on tables produced to the Chemical Section at Swansea, showed the efficient power of three generally used forms of battery, known as Smee's, Daniell's and Grove's, would be equal when 100 pairs of Smee's, 55 pairs of Daniell's, or 34 pairs of Grove's were used; and that the expense of working such batteries, as regards a standard of 60 grains of zinc in each cell per hour, would be about 6d., 7½d. and 8d. respectively.

On the Presence of Nitrogen in Mineral Waters. By W. WEST, F.R.S.

In this paper the author corrects the statement of Dr. Granville, in his 'Spas of England,' that the continental chemists do not find nitrogen gas in their analyses of mineral waters; whence the Doctor infers either some extraordinary difference between the spas of England and of the Continent, or some error in the experiments of British chemists. Mr. West showed, by quotations from many statements, principally of German chemists, that they at least, in many instances, state the proportion of nitrogen found by them, and that in those cases where this is omitted, the absence of nitrogen is not to be inferred, but only that they made no examination of the gaseous contents, beyond ascertaining the quantity of carbonic acid present.

On the Presence of Fluorine in the Waters of the Firth of Forth, the Firth of Clyde, and the German Ocean. By GEORGE WILSON, M.D., F.R.S.E.

In 1846, the author announced to the Royal Society of Edinburgh the discovery of fluorine as a new element of sea water. He was led to search for it, after observing that fluoride of calcium possesses a certain small but marked solubility in water, which explains its occurrence in springs and rivers, and necessitates its occasional, if not constant presence in the sea. The only specimens of sea water he had examined before this summer were taken from the Firth of Forth at Joppa, about three miles from Edinburgh. He obtained the mother-liquor, or bittern, from the pans of a salt-work there, and precipitated it by nitrate of baryta. The precipitate, after being washed and dried, was warmed with oil of vitriol in a lead basin, covered with waxed glass having designs on it. The latter were etched in two hours as deeply as they could have been by fluor-spar treated in the same way, the lines being filled up with the white silica separated from the glass.

The author has recently examined in the same way bittern from the salt-works at Saltcoats in the Firth of Clyde, but the indications of fluorine were much less distinct than in the waters on the east coast. On procuring, however, from the same place, the hard crust which collects at the bottom and sides of the boilers used in the evaporation of sea water, he found no difficulty in detecting fluorine in the deposit. This crust, or deposit, consists in greater part of sulphate of lime and of carbonate of lime and of magnesia; but it contains also much chloride of sodium, and the other soluble salts of sea water entangled in its substance. When sulphuric acid, accordingly, is poured on it, it gives off much hydrochloric and carbonic, as well as some hydrofluoric acid, and the latter is thus swept away before it has time to corrode the glass deeply. The author preferred, nevertheless, to use the crust exactly as he got it, that the proof of the presence of fluorine might not be impaired in validity by the possibility of that substance being introduced by the water or reagents which must have been employed, had the chlorides and carbonates been separated from the crust by a preliminary process. The crust, accordingly, after being dried and powdered, was placed along with oil of vitriol in a lead basin covered by

a waxed square of plate-glass, with letters traced through the wax. A single charge of the crust and acid corroded the glass only slightly; but by replenishing the basin with successive quantities of these materials, whilst the same plate of engraved glass was used as the cover, he found no difficulty in etching the glass deeply. The author is indebted to his friend Mr. S. Macadam for this simple but effective way of increasing the corrosion of the glass, which seems worth the adoption of chemists in all cases where fluorine is sought for. Four charges of material have been sufficient, with all the specimens of sea-water deposit he has examined, to mark the glass strongly. It was kept wet on the upper side, and exposed undisturbed to the action of each charge during twelve hours. Operating in this way, he has found fluorine readily in the boiler-deposit from the waters of the Firths of Forth and Clyde. It is a less easy matter to subject the waters of the open sea to the requisite concentration before examination. It occurred to the author, however, that the incrustations which are periodically removed from the boilers of the ocean steamers would serve to determine the question whether fluorine is a general constituent of the sea. He made application, accordingly, at Glasgow and Leith for the deposits in question. It appears, however, that the deep-sea steamers which leave the former have their boilers cleaned out at other ports, so that he has as yet been unsuccessful in procuring crusts from the west coast of Scotland. He has obtained at Leith the crust from the boiler of a steamer called the 'St. Kieran',* which trades between that port and Montrose; so that the greater part of the water consumed as steam by its engines is derived from the German Ocean, although a portion is necessarily obtained from the Firth of Forth. The crust from the boilers of this vessel was treated in the way described, and at once yielded hydrofluoric acid. A single charge, indeed, of the materials marked the glass distinctly, and four charges deeply. We may therefore infer that fluorine is present in the waters of the German Ocean, for different portions of the deposit yielded it readily, and marked glass as deeply as the deposit from the water of the Firth of Forth did, which could not have been the case if the whole crust had not contained fluorine pretty equally diffused through it.

It will be an interesting matter to have similar examinations made of the boiler deposits from the Transatlantic, and other ocean steamers which make long voyages; nor will it be difficult, where the crust is thick, to select portions from the interior of the deposit, which may be regarded as best representing the contents of the sea at a considerable distance from land. From what is known of the comparative uniformity in composition of sea water, it may safely be inferred that if fluorine be present in the waters of the Firths of Forth and Clyde and in the German Ocean, it will be found universally present in the sea. In one of the interesting communications which Prof. Forchhammer has laid before the British Association, he has shown that the more marked ingredients of sea water vary little over wide areas. One of the ingredients selected by this gentleman to mark the uniformity in composition of the sea, is lime, and as it is exceedingly probable that the fluorine in sea water exists in the state of fluoride of calcium, his observations may be referred to as in harmony with the inference that the element in question is generally diffused through the sea. Other proofs, however, are not wanting. Mr. Middleton, before 1846, came to the conclusion that fluorine must be present in sea water, since it occurred, as he had ascertained, in the shells of marine mollusca. Silliman, jun., without a knowledge of Middleton's views, drew the same inference, from its invariable presence in the calcareous corals brought to America by the United States' expedition from the Antarctic seas. The author has found fluorine abundantly present in the teeth of the walrus, which points to its existence in the Arctic Ocean; and it seems so invariably to associate itself with phosphate of lime, that it may be expected to occur in the bones of all animals marine and terrestrial.

The author has found fluorine likewise in kelp from the Shetlands, but much less distinctly than he anticipated. Glass plates were only corroded so far as to show marks when breathed upon. Prof. Vœlcker also was kind enough, at the author's request, to search for fluorine, when analysing the ashes of specimens of the sea pink

* In the account of this paper contained in the Athenæum report of the meeting of the British Association for 1849, the name of the vessel was inadvertently called the 'Isabella Napier' instead of the 'St. Kieran.' They both traded between Leith and the northern parts of Scotland.

(*Statice Armeria*), which had grown close to the sea-shore and contained iodine, and found fluorine in the plant.

When all those facts are considered, it is not too much, the author thinks, to urge that fluorine should now take its place among the acknowledged constituents of sea water. He has entered at length into the consideration of the natural distribution of this element, and into other details connected with it, in a paper in the Transactions of the Royal Society of Edinburgh, vol. xvi. part 7, and in a communication made to the Association at its Southampton meeting. The author further notices, incidentally, that the only ascertained plant, so far as he knows, in which fluorine had previously been detected, is barley, in which Will found it. In 1846 the author detected this element in American potashes, and it now appears to be one of the constituents of the kelp sea-weeds, although the observations which were made on commercial kelp are liable to the objection, that the fluorine detected might be derived from sea water which had dried upon the kelp weed before it was burned.

The *Statice Armeria* may certainly be added to the list of plants containing fluorine, and so may the *Cochlearia Anglica*, in specimens of which obtained from the Bass Rock, and analysed in Dr. Wilson's laboratory, Dr. Vœlcker has also detected this element.

Specimens of etched glass were shown to the Section in illustration of this communication.

P.S. The specimens of etched glass sent, are seen to most advantage if placed on a sheet of paper and held in direct sun-light, or any other bright flame, so that the shadows of the grooves which form the letters may fall upon the paper.

Analytical Investigations of Cast Iron. By F. C. WRIGHTSON.

This series of analyses showed the influences of the hot blast in producing the so-called "cold short iron," by occasioning an increased reduction of phosphoric acid, and the consequent increase of phosphorus in the "hot-blast" iron. The respective per-centages were:—

	I.	II.	III.	IV.	V.	VI.	VII.
Cold blast	0.47	0.41	0.31	0.20	0.21	0.03	0.36
Hot blast	0.51	0.55	0.50	0.71	0.54	0.07	0.40

The irons differed also considerably as to the state in which the carbon was contained, the hard white iron resembling impure steel, containing nearly all its carbon in a state of chemical combination, whilst the carbon contained in the gray and mottled varieties of iron was principally contained only as a mechanical mixture. The presence of sodium and potassium in all the specimens examined was also noticed for the first time, and it was thought probable that these might materially affect the qualities of the metal.

GEOLOGY AND PHYSICAL GEOGRAPHY.

Notes on the Geology of the Channel Islands.

By ROBERT A. C. AUSTEN, F.R.S.

THE object of the present short communication is not to give a detailed account of the mineralogical character of the various crystalline rocks which form so large a portion of this group, nor to lay down their topographical extent. The publication of facts of this class does not form any part of the objects of the British Association, and all that I would now attempt is a few general results, for the purpose of discussion, on one or two points in geological investigation, which these islands help to elucidate.

The mineralogical constitution of Guernsey in particular, as is well known, was investigated by Macculloch, himself a native of that island.

From the position of this group with reference to the coast of France, it is obvious that comparisons must be instituted rather with the formations of the Cotentin, than with anything on the English side of the channel. One great difficulty which every

one must have experienced in attempting to investigate the relations of the mineral masses of the Cotentin, arises from the want of natural sections, owing partly to the manner in which its surface is covered by heath and wood, and partly to superficial accumulations; a difficulty, which, though it exists to considerable extent in the larger islands of Guernsey and Jersey, is obviated by the great extent of coast-line they present.

The sedimentary rocks of Guernsey and Jersey are of inconsiderable extent; a small patch of clay-slate occurs in Rocquaine Bay, on the west of Guernsey, and larger areas are occupied by it in the north and north-east parts of Jersey: in the latter they are occasionally siliceous, and pass into subordinate beds of rounded conglomerate. The whole of this group has been variously moved about, but its general slope as a mass is to east. These beds are evidently a part of the palæozoic series of the Cotentin, and closely resemble that portion of it which, consisting of alternations of compact sandstones and argillaceous shales, are well seen on the north and south of Valognes. The calcareous bands of the French series are altogether wanting, nor did I see any of those peculiar steaschist beds, with nodules of quartz, which in the neighbourhood of Cherbourg underlie the middle part of the group.

Organic remains, if not entirely wanting, must be exceedingly scarce in these beds, in which respect they agree with that part of the French series to which I have compared them.

No trace of any one of the secondary series of formations is to be found over these islands; the surface of the slate rocks has been much denuded, and the like process may have removed whatever newer strata may have at some time existed there; but from certain characters which the new red sandstone and the cretaceous beds put on in their extension into the west of France, it is more probable that beds of that period never were deposited here.

The geological interest which attaches to these islands consists in the relative ages of the crystalline rocks, which form so large a portion of their masses. A circumstance which cannot fail to strike any observer is the very great changes of mineral character which these masses put on, and within very narrow limits: there is, however, a threefold division, which is apparent enough:—

1. A flat-bedded crystalline group, such as that which occurs over the southern half of Guernsey.

2. A granitic group, which includes a series of gray, red, and black granites.

3. A sienitic series, comprising a vast variety of combinations, in which, however, hornblende prevails.

The first of these groups, as it is seen in Guernsey, is of great thickness, and though no topographical limits can well be drawn, it may be said to occupy all south of a line from Castle Cornet to Vason Bay; it is in some places a true granite, at others gneiss, at others a true micaceous schist. It agrees with the next group as to the constituent minerals into which it graduates downwards.

The true granites are to be seen over the northern part of Guernsey, nowhere better than in the quarries of St. Sampson's parish. These latter, as a mass, underlie the first group, and their more massive external character, as well as more uniform crystalline texture, may be merely the results of cooling under rather different conditions; and the whole may be an intrusive plutonic mass of the same period. On the other hand, portions of the upper group irresistibly suggest the notion that at some time they must have existed as sedimentary strata.

In the island of Jersey the true granites occupy the southern portion, and it is only here that we see their relation to the slate series already noticed. As the slates approach the granite, they become hard and splintery. At this junction enormous veins or branches extend from the granitic mass, as well as the most delicate threads; at places the slate rocks seem reduced to fragments, among which the fluid granite has poured itself, the angular edges being sharp and uninjured.

The granite of this island puts on a character more closely resembling stratification than is usually seen; so much so, that in many places, as for instance in the steep walls of rock beneath the citadel of St. Heliers, it might be excusably mistaken for a mass of highly inclined sedimentary beds. These divisions have no general angle of dip, but are inclined most unequally; they may be planes of cooling which were once horizontal, and have acquired their present position by subsequent disturbance. The extreme smoothness of their surfaces is very remarkable.

In the red granites I observed one set of planes running nearly north and south, with a dip to the west and a cross set east and north, and which had a dip north.

Hornblende is not absent in the second group of crystalline rocks, as an occasional constituent; and in these cases, as in St. Sampson's parish, it makes its appearance by gradual increase, and as it were by passage from one rock to another.

The third group is quite distinct: the different appearances which it assumes, from the preponderance of one or other of its constituents, would cause it to be described mineralogically under a great variety of names. I noticed, however, a single dyke in the island of Jersey which in one place was an earthy hornblende (wacke), then compact greenstone trap, hornblende with distinct crystals of felspar; lastly, hornblende, with large plates of mica.

The intrusion of the hornblendic series is of subsequent date to that of the granitic rocks: as to the period in the geological scale at which this took place, it would be hazardous to conjecture; they have broken up and been projected amongst the granites, in the same manner as we have seen that the granites affected the slate rocks. A vast lapse of time must have been required for the cooling down of the fluid granitic masses; yet it is evident that the whole of that structure of divisional planes was complete before the intrusion of the sienitic rocks; an illustration of this represented the dyke in every instance following one of these sets of planes. But for numerous other sections, where the granite is seen caught up in the greenstone in great angular masses, it might be supposed that the two rocks were arranged in parallel beds; instances of the subsequent date of the hornblende series is perhaps best seen in Jersey, but good examples are to be found in Guernsey.

At several places in this same island is to be seen a deposit of fine sedimentary matter, conforming to the irregular surface of rock on which it rests. These accumulations have for a few years past been worked for the purposes of brick-making, so that good sections can be obtained. Their position is on the high table-lands of the south part of the island, so that at the time of their deposition the whole must have been submerged; but besides this, the beds themselves would indicate a great depth of water. No fossils that I could ascertain have ever been met with. In one or two of the lower portions of this deposit, and where the sands were rather coarser, I detected fine sharp angular fragments of chalk flints; and guided by many considerations which it would be needless to mention here, but which will readily suggest themselves to those acquainted with the geology of the south-west parts of France, it seemed to me that these beds might be outlying patches of the deep sea eocene period.

The geological phenomena of these islands next in date are referable to *sub-aërial conditions*—the deep disintegration of the crystalline rocks, and the accumulation of the materials so produced. The thickness of these accumulations indicate a long lapse of time; they cover not only all portions of the larger islands, but are found capping the smaller groups of rocks which surround them: they come down to the present sea-level; they evidently, by their position, belong to a period when the whole of those islands had a much greater amount of elevation than at present.

The old peat-beds and forest-trees of Catel parish belong to this period of sub-aërial conditions, as do also the submerged forests which run out from these islands at so many places, Vason Bay, Grand Cobo.

The elevation of the whole of this group at this line was probably very considerable.

Up to a height of no great amount above the sea, the surface is covered by an accumulation of sharp sand, with occasional lines of shingle; chalk flints enter largely into the composition of this. In the parish of St. Sampson it will be seen resting on the surface of the granite, as in many of the quarries; but it occurs equally on the *sub-aërial beds*; the thickness of these accumulations is very trifling, and can only indicate a depression beneath the present level of very transient duration. In the Island of Jersey such lines of inland cliff as that which extends from Gorey southwards, at the base of which lie the ancient marine beds, covered along the sea-board with blown sands, would indicate a rather lengthened period of stability before the last change of level.

Such is the series of physical change which this group of islands appears to have undergone; its geological history is simple compared with many other districts, but for the apparent fact that it should have preserved tracts as dry land through so many surrounding changes, and probably since the post-eocene period.

On some New Species of Testacea from the Hampshire Tertiary Beds.

By E. CHARLESWORTH, F.G.S.

Mr. Charlesworth stated that the British Natural History Society had employed collectors to obtain fossils from the eocene strata of Hampshire, and that amongst the 20,000 specimens already obtained were seven new to this country:—1. A *Cytherea* with the external form of *Isocardia*. 2. A *Purpura*? with a single prominent plait on the columella. 3. A *Cancellaria*. 4. A shell allied to *Cerithium*. 5. *Murex tripteroides* (Desh.). 6. *Fusus excisus* (Lam.). 7. A variety of *Murex defossus* (Sow.). Mr. Charlesworth remarked on the importance of investigating particular deposits, especially where there was any danger of good localities being destroyed, as in the case of the Bridlington crag.

On the Geography and Geology of the Peninsula of Mount Sinai and the adjacent Countries. By JOHN HOGG, M.A., F.R.S., F.L.S., Hon. Sec. of the Royal Geograph. Soc. &c.

In communicating a brief account of the geography and geology of the peninsula of Mount Sinai, and of the countries immediately adjoining to it, the author in the first place took a hasty survey of the chief natural features of the peninsula, beginning at Suez, and following the Sinaic coast of the Gulf of Suez as far as its south point at Ras Mohammed, and thence up the Sinaic coast of the Gulf of Akaba to its north extremity.

Secondly. From the Kalah el Akaba down the Arabian shores of that gulf, he described that region, the little isles of Tiran, Senafer, and others which lie to the S. of Ras Furtak, and then the districts near Ain Uneh and Moweilih, on that coast of Arabia.

Thirdly. Passing from Moweilih up the Gulf of Akaba, he gave some views of it, of the Wadi el Araba, and of the neighbouring mountains, as far north as the ruins of Petra.

Fourthly. On the rocks of Petra the author offered a few remarks, also on Gebel Harun, and the mountains of the Nabathæan chain, those to the N.W. of Wadi el Jerafah, the great desert of El Tyh, the range El Egmeh, the Sinaic group, and Gebel el Tyh and G. Thugar.

Fifthly. Starting again from Suez, he shortly noticed that east region of Egypt which is contiguous to the Gulf of Suez, nearly as far south as the supposed site of Myos Hormus.

And, sixthly. In conclusion, he observed upon the general features, the heights of the mountains, the geological formations, the minerals and ores of the peninsula of Mount Sinai.

The plain map that accompanied this paper was carefully reduced from a much larger one (which was also exhibited and coloured geologically), drawn and compiled by the author from the maps of Professors Lepsius, Russegger and Robinson (the last executed by Kieppert at Berlin), and from the charts of the survey of the Red Sea, by Messrs. Moersby and Wellsted, under the authority of the East India government. For the purpose of keeping the map as clear as possible, and not crowded with names, those of the chief places are alone inserted. The Arabic, the classical and scriptural appellations are added. It was recently engraven by Mr. W. Hughes for the Royal Society of Literature, in order to illustrate the author's previous memoir on Mount Sinai, now publishing in the forthcoming part of their Transactions*.

Mr. J. Hogg also exhibited a copy of the same, which he coloured geologically, principally after Russegger's maps of Egypt and the Sinaic peninsula, very lately executed at Vienna; but the latter he corrected in some places according to the descriptions of Burckhardt and other travellers who had visited them in person.

An imaginary section, likewise geologically tinted, was described; it comprehended the peninsula from Gebel Jaraf on the north to Ras Mohammed on the south. This the author himself enlarged, eight times, from a portion of a more extensive section, neatly engraved with the altitudes derived from Russegger's work, by Herr Augustus Petermann.

* See Second Series, vol. iii. part 2. pp. 183, 236.

Two other geological sections, which the author sketched and coloured, were also explained; the first was a representation of the 'Granite Peaks of the high Sinaic mountains,' enlarged after Russegger; and the second was entitled, 'Section of the Wadi el Araba, from the Gulf of Akaba to the Dead Sea, showing what portion is *now lower* than the *level* of the Red Sea.' He likewise stated that the stoppage of the River Jordan through that Great Wadi (supposed to have once flowed through it) might have been effected by a *volcanic* agency, traces of which exist about the Dead Sea, and near the head of the same gulf.

It is impossible in the limits of the present abstract to follow the author through his several divisions, wherein he carefully recorded the chief facts relating to the rocks, mountains, and plains, and the nature of the respective formations. But some of the geological characters, and the different formations of these countries, as far as they are at present known, are the following. —

- I. Diluvium, alluvium, sand, marine formation, coral rocks, &c.
- II. Tertiary sandstone, upper Nubian sandstone, and oldest diluvium.
- III. Tertiary limestone and marl.
- IV. Limestone of the cretaceous series.
- V. Older sandstone, Nubian sandstone, and its marl (lower cretaceous series).
- VI. Unstratified or crystalline rocks; granite, sienite, porphyry, diorite, greenstone, felspar, gneiss, chlorite, hornblende, mica and clay-slates, &c.
- VII. Volcanic rocks; basalt, and basaltic lava.

The distribution of these formations over these regions of Arabia and Egypt is briefly thus:—

A large tract of beds comprised in I. occurs around the head of the Gulf of Suez and to the N.W., where exist the salt marshes, Szabegha. Then due N. a strip of tertiary sandstone and oldest diluvial beds, II.; next, a narrow piece of tertiary limestone and marls, III.; again a large extent of II., interrupted by a narrow belt of III. running N. and S., which stretches out N.E. nearly to 34° E. long. From thence the immense desert of El Tyh with its many plateaux of different elevations, bounded by Gebel el Rahah on the W., the Gebel el Tyh range on the S.W., S., and S.E., nearly to the line of 29° N. lat. consists of IV., limestone of the cretaceous formation, but covered in places by large tracts of sand, gravel, and flints. The western coast of the peninsula is I., about as far as Ras Soddur from the head of the Gulf of Suez on the W.; but to the E., including more than half the range of El Rahah, III. prevails. Between that Ras (cape) and Wadi el Amarah, there intervenes an outlying piece of IV. From the last valley (Wadi) to about El Hamam Faroun, III. again comes in, which continues a little to the E. of Howara. From Wadi Gharandel to the N. of Wadi Naszb and the well of Morkha, except along the sea-shore and the plain W. of the latter spot, which are of I., bounded on E. by Gebel Watah, and from thence to W. Naszb in a S.W. direction, IV. extends. From that mountain to Sarbut el Chadem inclusive, and from Morkha on the coast plain to the head of El Kaa, below Mount Serbal on the W., the sandstone (secondary), V., and its marls occupy that district. Gebel Araba range, near the sea, is of limestone, IV. The long gravelly and sandy plain of El Kaa, which stretches out to the S. extremity of the peninsula is I., and more or less covered with pebbles and detritus of the primitive rocks, VI. Along the coast there follows a small chain, including the remarkable G. Narkus of V., then succeeds G. Hemam, nearly as far as Tur Bay, composed of IV. Two patches of III. occur N. and S. of Tur; but that small town, the only one in the peninsula, stands on a raised coral bank and sand, I. South of Ras Sebil there is a little tract of III.; and this reappears at Ras Mohammed; N.W. and N.E. of which low promontory some older sandstone, V., intervenes between it and the granitic roots of the Gebel El Turfa. East of Sherm, which is of V., volcanic rocks, VII., are seen, and crater-like appearances. Thence north-eastwards, V., where an intermediate strip of IV. is found at Wadi Nubk. Along the Sinaic coast of the Gulf of Akaba up to Noweibia from Wadi Orta inclusive, VI. prevails; a little of V. occurring N.W. at Dahab, and in the lower part of W. Sal.

The unstratified or crystalline rocks, VI., range from the S.E. of Sarbut el Chadem, bounded by the S.W., S., and S.E. sides of the elevated sand plain of V., called Debbet el Ramleh, and from Wadi Ronman, and the N. end of W. Firan, where it joins W. Mukatteb, along the eastern edge of El Kaa, to the S. termination of the El Turfa chain. Then N.E. of Wadi Sal, and N. of it to the northern branch of

El Tyh V. continues. Along the Sinaic shores of the sea of Akaba, from Noweibia, near which place is IV., the same extends northwards; somewhat to the west and north of this coast line, rocks of VI. and V. alternate, and occasionally with IV. exhibit many remarkable displacements; W. of the granitic Isle of Kureiyeh are black basaltic cliffs, VII., along the beach; then N. some breccia or conglomerate is noticed; and afterwards granite rocks succeed.

Ascending the Wadi el Araba, the mountains on the E. are of VI., chiefly porphyry with granite in places, to about 30° N. lat.; near which occurs the watershed, at about an elevation of 500 feet above the sea in that Wadi, the inclined bed of which, from the gulf to that point, is formed of sand and gravel and debris. Sandstone, V., and some chalky limestone, IV., are met with on the W. side of the Great Wadi Araba. Those formations are elevated to about the level of the desert El Tyh, and in spots somewhat higher. From about the line 30° N. lat. V. extends northwards beyond Gebel Harun and the ruins of Petra, both inclusive,—except an intermediate strip in Wadis Gharundel and Dalegheh running nearly W. and E., which is IV., and all along the E. side of this region, a lofty chain, attaining an altitude of perhaps 3600 feet above the sea, which the author termed the 'Nabathæan chain,' and proceeds a great distance north—consists of IV. On the other, the western side of the valley of the Araba, and opposite to Gebel Harun (Mount Hor), the abrupt Gebel Makrah and the peak of Gebel Araif el Naka, or the 'She Camel's Crest,' are likewise of the IV. formation. South of these begins "the great and terrible wilderness" of El Tyh, or 'the Wandering,' which has been already noticed.

On the east of the Nabathæan chain, as also of the granitic range of Mount Seir south of the line 30° N. lat., for a vast distance in the eastern desert, and to the S., the limestones of IV. extend. The mountains from Kalah el Akaba, the 'Castle of Akaba,' along the Arabian coast, are granitic, VI. At the promontory Ras Furtak is a low tract of IV. corresponding with that in the N. side of the opposite isle of Tiran, and with that in Wadi Nubk on the Sinaic shore. The coast then, south of the granite mount Gebel Makna, in many places is of I., but between those and the granitic range behind Ain Uneh and Moweilih, the tertiary sandstone II. is, according to Russegger, again developed. Further inland, the sandstones, V., of the lower cretaceous series prevail.

Again, on the coast of Egypt, S. of Suez, Gebel Ataka, which is of the limestone IV., divides a tract on the N. and S. of tertiary limestone, III.; the plain El Baidea being of I. S. of this a very considerable district of the secondary limestone IV. follows. On the heights above Wadi Zafraneh on the N., a strip of granite, VI., takes place, wherein exist traces of old copper mines. Near the coast S. of Ras Zafraneh, some beds of tertiary limestone and marl, III., and some conglomerate rock, are found. In the Eastern Desert, a little S. of $28^{\circ} 30'$ N. lat. and about $32^{\circ} 30'$ E. long., there occurs some of the sandstone, V., of the lower cretaceous series, and called by Russegger 'sandstone of Nubia.' The mountains rising between that portion of the desert and the coast are for a great distance southwards primitive or granitic, VI.; of these Gebel Garib or Agrib is the loftiest; its summit being elevated to about 6000 feet above the sea level. Both N. and S. of it are observable remains of copper mines.

The fossils of these tertiary and secondary formations have not as yet been sufficiently examined. Capt. Newbold states that he found among the many fossils of the limestone, IV., *Ostrea*, *Echini*, *Madripora*, and *Pectines*; and Herr Russegger observes, that in the compact chalk rock of the same series, IV., at Ras Hamam in the Gulf of Suez, he observed remains of monocotyledonous plants; and in the same formation, from the Gebel el Tyh, that is to say, compact chalk with flints, were numerous fossils. Mr. John Hogg however conceived it probable that some of the limestone formation, which Russegger assigns solely to the cretaceous series, in the vast district to the N. and E. of Gebel el Tyh, will, on further examination, prove to be of an older limestone. So he thought that certain of the sandstones of V. may, after future discoveries, be more correctly referred to rocks anterior to the secondary, perhaps to the Palæozoic, epoch; indeed in the older or secondary sandstone, V., Capt. Newbold could find no fossils. And with regard to the ages of the primitive or unstratified rocks, VI., of the Sinaic group, the same traveller, who examined them recently with care, says that the greenstone is the latest, and next in order the porphyry and granite, and that the hypogene schists or slates are the oldest.

Few minerals and ores occur in the Sinaic peninsula; of these iron and copper are the most abundant; indeed, in hieroglyphics, Professor Lepsius remarks, that the whole country was called Mafkat, i. e. "the copper land." Neither lead nor silver has been detected, but near Mersa Dahab, which means the 'gold port,' some assert that gold dust is present, for the teeth of the Ibex are sometimes seen surrounded with it. This probably may be only auriferous pyrites. Hematite, antimony, rock-crystal, cinabar, nitre, rock-salt, a yellow clay named *tafal*, crystallized sulphate of lime, sulphur, gypsum, pebbles of agate and jasper, occur. Thermal springs rise at Gebel Hamam and in El Wadi near Tur; the former having a temperature of 55° R., and the latter 91° Fahr. The porphyries and granites of the high Sinaic group vary extremely in colours, and some are of great beauty; the latter resembling those near Assouan.

According to Russegger, the highest peaks of that group, in fact of the entire peninsula, rise to 9300 English feet above the sea. A peculiarity in the lower mountain ranges is this:—generally an ascending valley (Wadi) leads up to the summit, which constitutes a plain, and then another Wadi slopes down to the level of the neighbouring district. Such is even the *present* general form of the long Wadi el Araba.

The minerals and ores in Eastern Egypt are, the author believes, only iron, copper, and much naphtha or petroleum found at Gebel el Zeit, 'Mount of Oil;' and in that part of Arabia which comes within this notice, little or nothing is known of its mineral products. The soil however in several localities is much more fertile, and more abounding in water, than that either in Eastern Egypt or in the Sinaic peninsula.

Mr. J. Hogg illustrated his observations with some beautiful lithographed views of Suez, of the mountains in the peninsula, of the head of the Gulf of Akaba, and the site of Petra, by Mr. David Roberts and the late Lieut. Wellsted.

On the Relations between the New Red Sandstone, the Coal-measures, and the Silurian Rocks of the South Staffordshire Coal-field. By J. BEETE JUKES, M.A., F.G.S.

The author commenced by remarks on the interesting question of what rocks lay below the new red sandstone of the Midland Counties, and after giving a concise sketch of the structure of that district, directed attention to the particular instance of the South Staffordshire coal-field. He stated that a point of great practical importance was the nature of the boundary faults of the midland coal-fields, whether they were true faults, or only old cliffs of coal-measures with the new red sandstone abutting against them. Having been engaged in the government geological survey of South Staffordshire, he wished to point out what results had been already arrived at. He showed that each of the three formations entering into the structure of the district (namely, the new red, the coal-measures, and the Silurian) were unconformable to the other; that this unconformability was rarely locally appreciable, the difference in the dip or strike being slight, but was shown by each of the superior formations resting on different parts of the inferior at different places. The nature of this unconformability was exhibited in the cutting of the railway near Dudley, where beds of coal-measure sandstone abutted against a cliff of Silurian shale 20 or 30 feet high, both formations being nearly horizontal. He then briefly described the boundaries of the southern portion of the South Staffordshire coal-field, showing that on the east the new red sandstone was brought down against the coal-measures by a true downcast fault; that the coal-measures were worked for some distance beneath the new red sandstone, but that they appeared to be suddenly thinning out in that direction near West Bromwich, and that a little east of the present workings the Silurian shale had been driven into, on a level with the thick coal; that Silurian shale had likewise been met with near the surface south of Oldbury, and that it was therefore probable that there was a space on the east side of the present coal-field about Sandwell and Smethwick, where the new red sandstone rested directly on Silurian shale without the intervention of any coal-measures, but that this space was not of any very great extent, from true coal-measures having been reached not far from the Stonehouse near Harborne, and near Aldridge east of Walsall. He then traced the western boundary from Wolverhampton to Stourbridge, which he showed to be probably a true "downcast fault to the west," more or less complicated by minor faults and branches which spread from it into the coal-field. Along the southern edge of the field from Stourbridge, south of Halesowen to Lappal and the neighbourhood of Harborne, he described the boundary to be formed simply by

the superposition of the new red sandstone on the coal-measures, the beds of the latter dipping gently to the south, and the former resting on them with *apparent conformity*. He believed that here would be found the true upper beds of the coal-measures, and the lowest beds of the red sandstone, as deposited in that district, but doubted the existence of any beds of passage from one into the other.

As a practical conclusion, he stated that while there was every hope that profitable coal-beds lay beneath the larger part of the new red sandstone plain of the Midland Counties and Cheshire, it would not be advisable rashly to commence a search for them, nor without competent direction and advice; that this advice and direction might eventually be hoped for from the Geological Survey of Great Britain under Sir H. De la Beche. He likewise added, that if he were now asked to fix a limit of depth at which the coal was probably to be attained beneath the new red sandstone, he should say *five or six hundred yards* was the least depth the speculator would probably have to sink for it.

See
p. 134. **On Traces of a Fossil Reptile (*Sauropus primævus*) found in the Old Red Sandstone. By ISAAC LEA of Philadelphia. (Communicated by Dr. BUCKLAND.)**

The object of this communication is to announce to the Society that I have discovered the footprints in bas-relief of a *reptilian quadruped* lower in the series than has yet been observed. On the 5th of April last, in the examination of the strata in the gorge of the Sharp Mountain, near Pottsville, Pennsylvania, where the Schuylkill breaks through it, a large mass of remarkably fine old red sandstone attracted my attention. Upon it I was astonished to find six distinct impressions of footmarks in a double row of tracks, each mark being duplicated by the hind-foot falling into the impression of the fore-foot, but rather more advanced. The strata here are tilted a little over the vertical, and the surface of rock exposed was about 12 feet by 6 feet, the whole of which surface was covered with ripple-marks and the pits of rain-drops beautifully displayed in the very fine texture of the deep red sandstone.

The six *double impressions* distinctly show, in the two parallel rows formed by the left feet on the one side and the right feet on the other, that the animal had five toes on the fore-feet, three of which toes were apparently armed with unguinal appendages. The length of the double impression is $4\frac{1}{2}$ inches; the breadth 4 inches; the distance apart in the length of the step of the animal 13 inches; across, from outside to outside, 8 inches. The mark of the dragging of the tail is distinct, and occasionally slightly obliterates a small part of the impressions of the footmarks. The ripple-marks are 7 to 8 inches apart, and very distinct, as well as the pits of the rain-drops.

The footmarks assimilate remarkably to those of the recent *Alligator Mississippiensis*, and are certainly somewhat analogous to the *Cheirotherium*.

The geological position of this reptilian quadruped is of great interest, from the fact that no such animal remains have heretofore been discovered so low in the series. Those described by Dr. King, in the great western coal-field, are only 800 feet below the surface of the coal formation (No. 13 of Prof. Rogers, the State Geologist). The position of the Pottsville footmarks is about 8500 feet below the upper part of the coal formation there, which is about 6750 feet, according to Prof. Rogers, and they are in the *red shale* (his No. 11); the intermediate siliceous conglomerate (No. 12) being stated by him to be 1031 feet thick. These measurements would bring these footmarks about 700 feet below the surface of the old red sandstone.

A mass of coal plants exists immediately on the northern face (upper) of the heavy conglomerate, here tilted ten degrees over the vertical, and forming the crest or "back-bone" of Sharp Mountain. This conglomerate mass is about 150 feet thick at the western side of the road below Pottsville. On the same road-side, about 1735 feet from these coal plants (south and directly across the stratification), is the face of the rock tilted slightly over the vertical and facing to the north. It is proper to state that the limestone of the old red sandstone exists here, about 2 feet thick, and underlies these "footmarks 65 feet."

On a New Species of *Labyrinthodon* from the New Red Sandstone of Warwickshire. By G. LLOYD, M.D., F.G.S.

After stating the unfrequent occurrence of the remains of this extinct genus of reptiles, more especially of other parts of the body than of the head, and having shown

that on comparison with the remains of other species already described there were good grounds for assigning to the fossil referred to, and illustrated to the Section by a lithographic drawing, the rank of a new species, to which he proposed to apply the name *Bucklandi*, the author proceeded briefly to point out the osteological features of the fossil. The specimen was described as consisting of the internal surface of the greater part of the bones of the cranium, presenting both orbits entire, the nasal aperture somewhat mutilated, and about twenty more or less perfect teeth in the superior maxillary bone of one side, and also preserving, either by the presence of bone or by impressions left of absent bones, the general configuration of the skull, the dimensions of which were about $11\frac{1}{2}$ inches from the termination of the premandibular to the extremity of the projecting condyles of the occipital bone, and about 9 inches from the outer edge of one temporal bone to that of the other. The general consolidation of the bones of the cranium, especially of those forming the orbits, was contrasted with the comparative loosely constructed skull of modern Batrachians; and the projecting condyles of the occiput were pointed out as highly characteristic of that family. The teeth presented the usual characters of the genus; and the position of the nostril, in conjunction with the other osteological peculiarities, confirmed the compound nature and amphibian habits of this reptile. The fossil described was recently discovered in the Bunter-sandstein.

Note on the Genus Siphonotreta, with a Description of a New Species. By
JOHN MORRIS, F.G.S. (Communicated by Sir R. I. MURCHISON.)

Among the numerous interesting fossils collected by Mr. John Gray from the Wenlock limestone and shale in the vicinity of Dudley, is a form which I am inclined to consider belongs to *Siphonotreta* (de Verneuil), a genus of Brachiopoda, hitherto considered peculiar to the Silurian formations of Russia.

The genus having been previously unnoticed in this country, and presenting some peculiarities both as regards the structure of the shell and the mode of attachment, it may not be uninteresting to offer a few general remarks on the subject; more especially as this shell, and some apparently allied forms, have been lately made the subject of a special notice by Dr. Kutorga of St. Petersburg. In this memoir* Dr. Kutorga has grouped together in one family (the Siphonotretæ) four genera, *Siphonotreta*, *Schizotreta*, *Acrotreta*, and *Aulonotreta*, which scarcely present any character in common, and have been in part considered by preceding authors as belonging to different groups or distinct subfamilies of the Brachiopoda.

Of the above-mentioned generic forms, two of them have been known for about twenty years. One of them, remarkable for the immense abundance with which it occurs in the lower Silurian grits of the north of Russia, its broken fragments disseminated in the plane of stratification, giving to the rock a micaceous appearance, was first made known (1829) as a peculiar genus by Prof. Eichwald†, under the name of *Obolus* (*Aulonotreta*, Kut.); about the same period (1830), Pander‡ gave the name *Ungula* to this fossil, and which L. von Buch§ considered to be an *Orthis*. The other form was also first noticed by Prof. Eichwald in 1829 as a *Crania* (*C. sulcata*, *C. unguiculata*), which he afterwards (1843) placed under *Terebratula*||; subsequently however M. de Verneuil, in the second volume of the great work on Russia¶, after a careful examination of these fossils, clearly recognized the differences which separated them from *Crania* and *Terebratula*, and gave them the very characteristic name of *Siphonotreta*, describing two species, *S. unguiculata* and *S. verrucosa*.

Since the publication of the work on Russia, four additional species of the latter genus have rewarded the researches of Hern. v. Volborth and other Russian geologists, and which are fully described, as well as those previously known, in the monograph by Dr. Kutorga above alluded to, and from which is extracted the following synopsis of the principal characters of the genera in the family.

* Über die Siphonotretæ, von Dr. S. Kutorga, Verhandlungen der Kaiserlichen Mineralogischen Gesellschaft für das Jahr 1847, p. 250.

† Zoologia Specialis, 1829, vol. i. p. 274.

‡ Beiträge zur Geognosie der Russischen Reichs, 1830.

§ Beiträge zur Bestimmung der Gebirgsformationen Russland, 1840.

|| Beiträgen zur Kenntniss des Russ. Reichs, 1843.

¶ Russia and the Ural Mountains, 1845, vol. ii. p. 286.

SIPHONOTRETEÆ, Kutorga.

A. With a tubular closed siphon.

- a. The external siphonal opening passes from the apex towards the anterior margin.
1. *Siphonotreta*, De Verneuil.
- b. The siphonal opening is directed from the apex towards the dorsal margin.
2. *Schizotreta*, Kutorga (*Orbiculoidea*, D'Orb.).
Opening narrow, slit-like; no area, nor mark of deltidium.
3. *Acrotreta*, Kutorga.
Opening elongated oval; area triangular and flattened, with a deltidium-like furrow.

B. With a furrow-like siphon, opened on the whole hinge plain.

4. *Aulonotreta*, Kutorga (*Obolus*, Eichw.; *Ungula*, Pander).

The author adds a series of critical remarks on the above groups, noticing some peculiarities of their geographical and geological position, and concludes by characterizing the new species of *Siphonotreta*.

SIPHONOTRETA? ANGlica.

Shell of a rather oval form, depressed, marked by fine lines of growth; surface minutely but concentrically reticulated, reticulation regular with quadrangular areolæ, and covered with many slender linear tubular spines or their bases, somewhat quincuncially arranged; spines smooth, dilated at the base, a little above which they remain of nearly uniform size throughout, and are regularly and transversely sulcated or contracted, giving the spines a beaded or jointed appearance.

The general form of this shell and quincuncial arrangement of the spines resemble *S. aculeata*, Kutorga; but as that author does not figure or allude to any reticulated structure or the moniliform spines*, this is considered to be distinct; unfortunately the specimen is compressed, so that all the characters are not fully shown.

On the Metamorphosis of certain Trilobites as recently discovered by M. Barrande. (Communicated by Sir RODERICK IMPEY MURCHISON.)

Sir Roderick Murchison brought before the Section the important discovery made by M. Barrande of the metamorphosis of Trilobites, as exhibited in a series of forms apparently very distinct, but which have been shown by that author to belong to the one species *Sao hirsuta* (Barr.). Referring in the first instance to the extraordinary number of species of Trilobites recently discovered in the palæozoic rocks of Bohemia, whether as compared with the small number hitherto known in that tract, or the whole quantity described in other parts of the world, Sir Roderick explained how with untiring zeal and ability, and at considerable cost and labour, M. Barrande had been the real agent in opening out this rich field, and how by a long and careful analysis of all its organic remains he had shown that it is essentially of Silurian age. In anticipation of a great work by M. Barrande (the 'Silurian Rocks of Bohemia'), in which the necessary proofs will be given, and containing among numerous other illustrations 40 plates of Trilobites only, Sir Roderick communicated the following extract of a letter from that author:—

"The fact, which is made intelligible by the plate of drawings annexed†, relates to a species which I have named *Sao hirsuta*, of which I have verified the gradual development from the embryonic to the adult state. I have been able to discover twenty successive stages in this progress, which took place after development from the egg, as is observed in some of our modern crustaceans. The first stage is marked by a disc two-thirds of a millimetre in diameter, of which the head only occupies the whole of the trilobed surface. In the second stage the thorax appears in a rudimentary state, and it increases in the following stages by the successive addition

* The moniliform character of the spines may not be peculiar to this species, but will probably be found to belong to the whole genus, when the spines are carefully examined by a higher power than that used by Dr. Kutorga.

† Of these an enlarged diagram by Mr. Salter was exhibited.

each time of a ring, until the thorax has thus acquired seventeen free segments, and the pygidium two anchylosed segments, in all nineteen, which constitute the adult age. During the course of this evolution the form of the different parts of the body is developed in so continuous a manner, that in tracing the successive stages there is no sort of 'hiatus.' Towards the sixth stage four isolated grains are observed on each side of the glabella. I name 'principal grain' that which is nearest to the axis, and 'primitive grains' the three other and smaller grains, which are arranged in a convex band towards the interior. Now, these four grains are persistent in all the following stages, both in their relative size and reciprocal position, with a constancy and regularity which alone might suffice to establish the specific identity of all these forms. The principal grain is also recognizable upon the adult, but at that age the three primitive grains become merged or lost amidst a crowd of other grains which accumulate around them. They all terminate in assuming a conical form, *i. e.* they become spines. [The figures show the details.]

"Three other species have offered to me an analogous development, but with fewer intermediary stages between the extremes. These are the *Trinucleus ornatus* (Sternberg), *Arionius ceticcephalus* (Barr.), and *Arethusina Konincki* (Barr.).

"The embryonic evolution out of the egg took place then in species or isolated genera among the trilobites of the Silurian epoch, just as among modern crustaceans. That which is remarkable is, that two of these four species belong to my lowest or primitive fauna, or to my band C, or the schists of Skrey, viz. *Sao hirsuta* and *Arionius ceticcephalus*. You know that the *Trinucleus ornatus* characterizes my band D, or your Caradoc sandstone *; and lastly, the *Arethusina Konincki* is found exclusively at the base of my inferior limestone E, which occupies the place of your Wenlock formation. In the three other superior bands of my upper division of the Silurian system, no trilobite has offered to me a trace of a similar evolution. These all appear to be born with the complete number of thoracic segments, but not with all the articulations of the pygidium †.

"I have thought that an acquaintance with this fact would be of some interest to you who first opened out the necropolis of trilobites.

* M. Barrande does not consider, with Mr. Salter, that the *Trinucleus Caractaci* (Murch.) is the same species as the *Trinucleus ornatus* (Sternb.).

† The comments made by the eminent naturalist M. Milne-Edwards, Member of the Academy of Sciences in Paris, on this communication, must have so much weight, that a deviation is made from the ordinary practice in giving this abstract of them in a note.—"Prof. Milne-Edwards remarked that this discovery was equally interesting to the zoologist and physiologist. Metamorphoses like those of the insect and tadpole were formerly supposed to be exceptions to the ordinary rule, until the researches of Harvey showed, that the chick in the egg underwent changes quite as extensive and remarkable. It now appears to be a law of nature, that animals are more alike as they are observed at a period nearer their embryonic state; and it is of the highest consideration in zoology to show, through what stages animals pass before arriving at their adult form. The zoological affinities of the trilobites were long a matter of dispute. They were first supposed to be Chitons, until Alexander Brongniart showed they were true crustaceans. But the crustacean forms are very varied, and it has been held uncertain whether the trilobites were allied to certain Isopoda, *sc.* Oniscus, or, as Mr. Thompson suggests, to Apus. Barrande's observations confirm the views of Mr. Thompson. The Isopods are born almost with the same form which they retain through life; but the Apus quits the egg in an imperfect state, having but few of the segments which constitute the body of the adult. In the young *Sao* the number of thoracic segments continually increased until the animal was adult; and to each of these (though no traces are now seen) legs were certainly affixed, not like the hard legs of insects adapted to terrestrial movement, but soft and membranous like those of Apus, for swimming in the water. The cephalic shield, which in the youngest stage of *Sao* formed the whole animal, constitutes but a small portion of the adult; and the amount of change exhibited in successive stages of development is so great, that it would be no wonder if zoologists should have built up upon it numerous species and even several genera. The rule which obtains now, that animals belonging to the same zoological type, though much differing in the adult, lose those differences in states approaching their embryonic condition, is seen even in the remains of animals which perished in the most remote epochs; and thus the tenants of the Silurian seas furnish arguments hitherto afforded by the study of living animals alone."

"How many times, in describing my species, do I think of your assertion, which the facts have so gloriously justified, 'that the Silurian system is the great centre of the creation of trilobites'! At that early epoch Bohemia seems indeed to have had the privilege of uniting an immense variety and multitude of these crustaceans; for the number of my species already exceeds 200. If you think this account of the metamorphosis of a trilobite of sufficient importance, announce it in any form you please to the British Association for the Advancement of Science.

"J. BARRANDE."

On the Distribution of Gold Ore in the Crust and on the Surface of the Earth.
By Sir RODERICK IMPEY MURCHISON, G.C.S., F.R.S. &c.

The recent discovery of considerable quantities of gold ore in California having excited the public mind, and led to some conclusions which he esteemed to be exaggerated, the author took this occasion of the meeting of the British Association to bring forward the whole subject of the distribution of gold ore over the surface of the earth, not merely to develop his own views and those of others, but also to elicit by discussion, the knowledge of the assembled geologists, mineralogists, miners and statisticians. An enlarged Mercator's projection of the world was exhibited, on which all the leading ridges which had afforded gold ore in times past or present, were marked, as taken in a great part from a general sketch-map by M. Adolphe Erman of Berlin, the explorer of Siberia and Kamschatka, which is appended to a geographical and mineralogical description of California by M. Hoppe and himself, as inserted in the 'Archiv für Wissenschaftliche Kunde von Russland' (7 band, 4 heft).

Referring to the works of Humboldt and Rose on the Ural Mountains, as well as to those of Helmersen and Hoffman, the former of whom constructed some time since a map of all the gold tracts of Siberia, and also citing the other contributions of M. Adolphe Erman on this head, Sir Roderick gave a condensed view of his own observations on the gold regions of the Ural Mountains. His exploration of that chain, in company with his associates M. de Verneuil and Count Keyserling, led him to form the opinion, that great and rich gold veins had alone been produced in the oldest formations, and chiefly where they have been highly metamorphosed by the intrusion of igneous rocks; in other words, that wherever clay-slates, old limestones, and greywacke sandstones (whether azoic or of Silurian, Devonian and Carboniferous age), had been penetrated by greenstone, porphyry, syenite, granite or serpentine, and were consequently in a more or less metamorphic or crystalline condition, there auriferous quartzose veinstones most occur, containing gold ore diffused in grains, leaves, lumps, and irregular filaments. Every discovery in the auriferous regions of Siberia and America, as well as all the workings in the Old World in past times, confirm this view, and prove it to be a *geological constant*, that the azoic and palæozoic rocks, when metamorphosed, are the only *great* repositories of gold ore. The minute quantities of auriferous pyrites and gold which have been detected in the secondary and younger deposits do not interfere with this generalization.

To the general view of Baron von Humboldt, that the richest gold deposits are those which are derived from ridges having a meridian direction, M. Adolphe Erman is decidedly opposed; but Sir Roderick is of opinion, that a much greater quantity of gold ore has been obtained from chains having a nearer relation to N. and S. than from those approaching to equatorial or E. and W. directions, due perhaps to the general form of the chief masses of land and the prevailing strike of the palæozoic rocks. He next pointed out an error into which some persons had fallen, of supposing that the chief Uralian mines were worked underground; the only small subterranean work being one near Ekaterinburg, which affords a very slight profit. All the rich mines along that meridian chain, throughout 8° of N. latitude, are simply diggings and washings which are made in the detritus or shingle accumulated on the slopes of the ridges and in the adjacent valleys, and with one small exception are all upon the eastern or Siberian side. This phenomenon in the Ural Mountains is a necessary result of their structure; the older and more crystalline formations through which the eruptive rocks have risen constituting chiefly the crest and eastern slopes of the chain, whilst the western slopes are occupied by deposits of younger or Permian age. As the conglomerates and de-

tritus of the latter deposits contain no traces of gold, though they abound in copper ores, it was pointed out by the author in his work on Russia, that the auriferous veins were there posterior to those of iron and copper, and must have been produced after the accumulation of the Permian system.

Exhibiting maps, sections and views of the Ural Mountains, formerly prepared by him, and referring to the description of California by Erman and others, he entered upon a comparison between the two countries, and showed that there were great coincidences of mineralogical structure in both, and that with these *constants* the same results obtained in America as in the Ural; the chief distinction consisting in the apparently larger proportion of gold in the detritus of the newly-discovered deposits of California than in those of the Ural. He contended, however, that no very large tract of California would be found to be as uniformly auriferous as the banks and slopes of the upper tributaries of the Sacramento. That gold ore has been found in certain localities along the western slope of the Sierra Nevada is admitted, but its continuity as well as the breadth of the deposit have yet to be ascertained.

And here the author took some pains to indicate the distinction between all such surface operations as those of Siberia, California, and the Brazils, and those works in which, besides the ores of silver, copper, &c., gold also had been extracted from veins in the solid or parent rock; the latter operation being very seldom remunerative. Adverting to the fact, that in the Ural Mountains the veinstones "in situ" (in this case little or no admixture with other ore exists) have proved very slightly remunerative when worked further downwards, he glanced at an opinion of Humboldt, who looking to the great lumps or "pepites" occasionally found in the surface rubbish, supposed that there may have been some connection between the production of gold and the atmosphere; since judging from these specimens, it was from the superficial extremity of these quartz veins that the richest bunches of gold must have been derived; the veinstones when followed downwards having invariably proved either sterile or very slightly productive.

The author carefully distinguishes the major part of the auriferous detritus from modern alluvia, and shows that it has been the result of former and more powerful causes of degradation than those now in operation—causes which distributed coarse shingle, blocks and sand, and which wearing away all the associated schists and the most oxidizable ores, left only the harder rocks, particularly the quartz veins, together with the harder and nobler metals gold and platinum. The existing rivers have had little more to do with this phenomenon than that in mountainous tracts; and where they have a rapid descent, they have occasionally laid bare the edge of the previously-formed and water-worn gold accumulations. By this observation it is not meant to deny, that where existing streams flow directly from rocks "in situ" which are now impregnated with gold, a little auriferous detritus must not naturally be washed down, but simply to prevent the student who may refer to detailed maps of gold tracts from imagining that the rivers are auriferous, except where they derive that quality from the wearing away and breaking down of the mixed materials which constitute their *ancient* banks. In a word, British geologists may be assured that gold shingle and sand have been accumulated just in the same manner as the former great drifts of their own country, whether general or local, in which bones of the fossil elephant, rhinoceros, and other extinct quadrupeds occur.

Having terminated his account of the geological *constants* which accompany gold mines in Europe, Asia and America, Sir Roderick then traced the history of gold and its development, as known to the ancients and our ancestors of the middle ages. He showed that in all regions where the above-mentioned palæozoic, crystalline and eruptive rocks occurred, gold had been found in greater or less quantities, and that just in proportion to the time a country had been civilized, the extraction of the precious metal had diminished; so that in many tracts, as in Bohemia, where gold had formerly prevailed to a great extent, it had been worked out and the mines forgotten. Briefly alluding to the examples at home of gold-works in Wales under the Romans, where Silurian rocks are pierced by trap, and contain pyritous veinstones as described by himself*, and to the former gold of Scotland and Ireland in similar rocks, its occasional discovery still in the detritus of the county of Wicklow, and its diffusion in

* Silurian System, p. 367.

some of the oldest Silurian strata of Merionethshire, he particularly dwelt on the continental tracts formerly so rich, as cited by Strabo, all of which (with the exception of the North Ural or country of the Arimaspes*, from whence, as Humboldt believes, the Scythian ores came) had been exhausted and were no longer gold-bearing districts. This circumstance is explained by the Scythian or Uralian gold having remained unknown from the classical age until this century. So completely ignorant were the modern Russians of the existence of gold in the Ural Mountains, or that they had in their hands the country which supplied so much gold to Greece and Rome, that excellent German miners had long worked the iron and copper mines of that chain before any gold was discovered. Even then gold was worked from a solid vein-stone for some time before the accidental discovery of gold ore in the ancient alluvium or drift led to the superficial diggings, which produced at an infinitely less expense the present produce. All the energy however displayed by the Russian miners has failed to augment the amount of Uralian gold much beyond half a million sterling, and as the period is arriving when the local depressions or basins of auriferous detritus of that region will be successively washed out, the Ural will then resemble many other countries in possessing actual mines of iron and copper, but a history merely of its gold. Russia, however, has also the golden key of all eastern Siberia, in which various offsets from the Altai chain (chiefly those which separate the rivers Lena, Jenisei, &c., or stretch along the shores of the Baikal lake) have proved so very productive in their gravel, that for some years they have afforded the enormous annual supply of upwards of three millions sterling, exclusive of the Ural.

As in the Ural Mountains, so has it proved to be in South America. There, the Spaniards, notwithstanding their keen search for gold from the days of Columbus to the present time, made many works in the parent rock, but either never discovered its existence or neglected to work it in the gravel and sand of the valley of the Sacramento, which tract they left in quiet possession of the native Indians. It was only indeed by the recent accident of the breaking away of a bank of detritus by a mill race, that this region was opened out for the first time to the colonists of the Anglo-Saxon race. What then is to be the value and duration of these Californian mines? On the point of absolute value the author does not venture to form an estimate in the absence of sufficient facts and statistical data; but in regard to the duration of this mining ground, he speculates that granting it to be locally much richer than similarly constituted detritus in the Ural, still there is nothing to interfere with the belief, founded on the experience derived from all other auriferous tracts, including those of Bohemia so productive in the middle ages, that, with the activity and numbers of the men now employed in the works, these deposits may in no great length of time be exhausted.

Judging from analogous facts, he is inclined to think, that the very great per-centage of gold ore in the gravel of the valleys of the Sacramento, indicates that the most valuable portions of the original veins have been ground down by former powerful denuding agencies; and if the rule be allowed which obtains very generally in mining, that the richer the veins the less are they likely to be spread over a large mass of parent rock, so is he disposed to think, that it will only be in certain patches that very great wealth will be discovered, and hence that it would be very wrong to conclude, that because rich gold detritus has been discovered on the affluents of the Sacramento, in lat. 40°, and also on the river Colorado in lat. 34° 5', all the intermediate tract of country should prove productive. Considering the vast addition in the few last years made to the European market by researches in Siberia, and seeing that such addition has produced no change in the value of gold as a standard, the author is of opinion (as far as the evidences allow him to judge), that the Californian discovery is not likely to produce any disturbance in the standard. At the same time he expresses his full agreement with M. Erman and others, that with the advancement of colonization in the central regions of North Asia, and other parts of the world where civilization has not yet extended, other gold tracts may be discovered, wherever the geological and lithological constants to which he has adverted occur; but neither

* If the gold tracts of the Ural Mountains had been explored and continuously worked from the time of Herodotus, they would have been exhausted ages before their occupation by the Russians.—R. I. M.

would this circumstance induce him to fear, that such discoveries (occurring probably at long intervals of time and for the most part in countries at enormous distances from the means of transport) will much more than compensate for the wear and tear of the precious metal, and the wants of a rapidly increasing population.

Sir Roderick then briefly alluded to the erroneous opinion of old authors, that the origin of gold had any reference to hot or equatorial climates, as testified by the abundance of ore in Siberia, even up to 67° N. lat., and cited a table of M. Erman, which showed, that by far the greatest quantity occurred in northern latitudes, there being every probability, that much more of this ore may be detected in the northern prolongation of the American chains and in the frozen regions of Russian America, just as it had been discovered in ridges of the north-east of Siberia and even near to Kam-schatka.

He reminded his auditors, that in considering the composition of the chief meridian ridge of Australia and its parallels, he had foretold that gold would be found in them; and he stated that in the last year a resident in Sydney (Mr. Smith), who had read what he had written and spoken on this point, had sent him specimens of gold ore found in the Blue Mountains, whilst from another source (Mr. Phillips) he had learned, that the parallel N. and S. ridge in the Adelaide region, which had yielded so much copper, had also given more undoubted signs of gold ore. The operation of the English laws of royalty had induced Sir Roderick Murchison to represent to Her Majesty's Secretary of State, that no colonists would bestir themselves in gold mining if some authoritative declaration on the subject were not made. The auriferous lines in Australia were marked in the general map.

In support of his general views, he called for the evidence of Professor William Rogers of Philadelphia, whose beautiful map of the Appalachian or Alleghany chain was exhibited; and he also fortified his inductions respecting the chief auriferous masses of Mexico and Peru by appeals to Colonel Colquhoun and Mr. Pentland, all these gentlemen being present. References were also made to an article by M. Michel Chevalier in the '*Revue des deux Mondes*' (1847), on the silver and gold mines of the New World as compared with those of the Old World; also to the work on the mines of Mexico by M. St. Clair Duport, to M. DufLOT de Mofras, to Macculloch's '*Dictionary of Commerce*,' and to Professor Ansted's '*Gold Seeker's Manual*.'

In conclusion, he specially directed attention to the distinctions between the two classes of gold-works, *i. e.* in the vein-stones and in their debris, and showed, that in the present day as in the remotest periods, the simple digging into and washing of old alluvial accumulations, have invariably proved to be the great source of production; whilst in works in the solid rock, on the contrary, the extraction of the gold from the silver alloy and other ores with which it is mixed up therein, and its separation from them, have proved so expensive, that to mine for gold as the Spaniards have done in South America, has frequently proved ruinous even to a proverb*.

On the Fossil Geology of Cornwall. By CHARLES WILLIAM PEACH.

The author commenced by noticing the extensive beds containing fish remains, which had been discovered since he communicated to the Section at Cork the few then found; that the beds enclosing these remains extend from near the Rame Head, Whitsand Bay, to the west side of Fowey, and that they are in places abundant; Bellerophonites also are rather plentiful, but each appear to have lived and died in separate flocks, rarely being intermixed with each other; a very few Gasteropods (*Loxonema*) are mingled with them. The beds generally rise at high angles, and are intermingled with trappean and quartzose beds, the line of strike nearly east and west, with a southerly dip, and appear to have been in places greatly disturbed.

Underlying these beds on the south side are a series of slaty, arenaceous and calcareous ones, containing Corals, Crinoids, Shells, Orthoceratites, a few Goniatites and Trilobites, some of these very abundant. These beds are first seen on the east side of

* The author expressed his regret at not being as yet acquainted with a geological work on California by the able American naturalist M. Dana, which had recently been announced.

Pencarra Point, and extend to beyond the Black Head, St. Austel Bay. Outside these are a series of hard quartzose rocks, commencing at the Cairn near Goran Haven, passing across to Caerhayes Beach, thence to Gerrans Bay; these contain Corals and Crinoids very rare, Orthides, and other bivalves more plentifully, and Trilobites not uncommon: there are also at these places small beds of limestone, a large series of conglomerates, in which are rolled blocks of limestone filled with crinoids and Orthoceratites; these rocks are a little out of the general line of strike. On the north side of these fish-beds are a very extensive range of fossiliferous ones, resting conformably on them; these may be traced from Whitsand Bay to St. Veep, and St. Winnow, and completely occupy the county *via* Bodmin, Liskeard, &c., to the sea on the north side. Although all the other organisms mentioned as occurring in the southern rocks are found in these, no traces of Trilobites have been noticed until reaching Bodmin; and at Menheniott, a bed exists there containing thousands. The author had also found organic remains rather plentiful at St. Columbporch, and at Newquay in the North Channel; at the latter place splendid Turbinolopsides, Crinoids, Trilobites, and a magnificent spine of an Onchus in clay-slates, associated with beds of impure limestone.

He remarked upon the very few fish remains that agreed with those found in the old red sandstone (one good specimen of *Asterolepis*, the species selected by Mr. Hugh Miller to illustrate 'The Footprints of the Creator') and those described in the 'Silurian System.' He concluded by saying that when Sir H. De la Beche made his survey of the county, only three or four places were known to be fossiliferous; now three-fourths of the county had been proved to be so, and in many places abundant: he trusted the day was not far distant when a new section would be run through the county, and the age of the rocks settled.

Notice of the Discovery of Beds of Keuper Sandstone containing Zoophytes in the Vicinity of Leicester. By JOHN PLANT.

In this paper the author describes the position of certain marls in the new red sandstone laid open by the cutting of the Leicester and Swannington railway, and the existence in them of markings in the sandstone which he refers to the genus *Gorgonia*. The sections show a thickness varying from 2 to 50 feet of superficial and detrital deposit, below which appear clays, marls, shaly marls and sandstones, offering a total thickness of about 200 feet, of which the first 150 feet contain masses and blocks, some of them weighing many tons, of the sienites, porphyries, and carboniferous limestone of Charnwood Forest and the neighbourhood. Amongst these are gray shaly sandstones containing the fossils developed between two beds of red clay which thin and swell out very irregularly. Between the sandstones are bands of fine marl enveloping the bodies described by the author as the polypidoms of a coralline, and these occur in great profusion on the surfaces of nearly every band, the bands being also furrowed by other markings. The polypidoms lie confusedly and in all instances occur as siliceous casts, the delicate organization of the cells being obliterated. Associated with them at times are thickly-set small granular concretions, giving the surface the appearance of shagreen.

The strata containing the fossils are considered to represent the keuper sandstone, both by their similar character and their distance from the lias. The author suggests for these fossil markings the name *Gorgonia Keuperi*.

On the Discovery of a Living Representative of a small Group of Fossil Volutes occurring in the Tertiary Rocks. By LOVELL REEVE, F.L.S.

In the Eocene portion of the tertiary series a small group of Volutes occurs, distinguished by a peculiarity of form and sculpture which is not found in any living species collected hitherto. The well-known *Voluta lima* of the British tertiary strata may be regarded as the type of this group; but there are other fossil species of the group which has been arranged as a subgenus by Mr. Swainson, under the title *Volutilithes*. During the late expedition of H.M.S. Samarang, a single living example of this type,

very closely and elaborately sculptured, and encircled by two or three coloured bands, was dredged by Sir Edward Belcher off the Cape of Good Hope, from a bank of dead shells, corallines, &c., at the depth of 132 fathoms.

All the species of *Voluta* hitherto known in a recent state are of comparatively solid structure, characterized by a copious deposit of enamel on the body whorl on reaching maturity, and none exhibit any detail of sculpture beyond that of longitudinal ribs. The species under consideration is not identical with any of the tertiary species, but of the same type more minutely latticed, similarly coronated, so to speak, and with a similar channeled excavation round the spire.

It is proposed to name it *Voluta abyssicola*, and it will be described and figured in the Mollusca of the Voyage of the Samarang.

Prof. W. B. Rogers exhibited the State Survey of Virginia, geologically coloured, and gave a general sketch of the structure of the country, with especial reference to the Faults in the Alleghanies. The State of Virginia comprises an area of 66,000 square miles, containing four distinct physical and geological districts:—1st, the Tertiary plain on the Atlantic; 2ndly, the rising ground consisting of gneiss, mica-slate and other primitive rocks, which lies between the coast plain and the Alleghanies, with the oolitic coal-field of Richmond occupying a depression on its surface; 3rdly, the Alleghany mountains; and 4thly, the great western coal-field. The Alleghanies consist of numerous parallel ridges of palæozoic rocks, ranging north-east and south-west, separated from the primitive region by the “Blue ridge,” a tract of igneous and highly altered rocks, which may be regarded as the igneous axis of the State. The anticlinal ridges of the Alleghanies all lean to the westward; and this want of symmetry increases towards the “Blue ridge,” until the strata forming the western flanks of each ridge are completely inverted, and dip under those on the eastern side; these great foldings and inversions of the strata are frequently attended by enormous faults, the western side of a ridge being absolutely engulfed and the eastern over-riding it; in these cases the Lower Silurian rocks sometimes rest on the inverted carboniferous limestone, and even on the conglomerates of the coal-measures: the displacement of the strata must amount in many instances to 10,000 feet; but if a fault is traced to a great distance either way, it is found to diminish gradually and terminate in a mere flexure of the strata; the length of the faults is sometimes more than 100 miles. Prof. Rogers then mentioned the occurrence of workable anthracite below the carboniferous limestone of the Alleghanies. In conclusion, he stated that during a recent tour in the Alps he had observed a general conformity in the structure of those mountains with the law of flexures exhibited in the Alleghanies; that is to say, the greatest dip of every anticlinal and synclinal was on the side furthest removed from the axis of disturbance: so that the general direction of the ridges and the curvature of the strata would now afford indications of the direction of the dynamic agency by which those flexures were produced.

On the Age of the Saurians named Thecodontosaurus and Palæosaurus.

By WILLIAM SANDERS, F.G.S.

The remains of these animals were discovered in the year 1835 by Dr. Riley and Mr. Stutchbury, who state that the dolomitic conglomerate in which they were imbedded forms the base of the new red sandstone, adopting the views announced by Dr. Buckland and Mr. Conybeare, in their Memoir of the Bristol Coal District. This memoir was published in 1822, accompanied by a map and sections, which represent distinctly the conglomerate rocks as constituting the lower division of the new red sandstone. The age thus assigned to these fossils was adopted by all geologists; it is so described in the best elementary works, and enters into the general statement made by Professor Owen in his Report on Fossil Reptiles. The Ordnance maps and sections present no alteration in this respect; they likewise represent the conglomerate as completely subjacent to the later new red.

Nevertheless the elaborate essay of Sir Henry De la Beche ‘On the Formation of Rocks in South Wales and South-western England,’ in the first volume of Reports of the Geological Survey, contains such a description of the new red sandstone beds as to lead the reader to concur with him in believing, that such conglomerates and limestones “may be of different dates,” and that “the cause of their production con-

tinued up to, and included the base of the lias." The author of that essay also notices certain tranquil deposits of red clays and marls on the surface of the carboniferous rocks. After making these preliminary remarks, Mr. Sanders exhibited a map of the parish of St. George's, near the mouth of the Avon, and another map of the three parishes of Compton Martin and West and East Harptree, together with sections for the purpose of illustrating the fact, that the spaces coloured on the Ordnance map as conglomerate, are really composed of several small tracts of conglomerate at different elevations, separated by larger tracts of tranquilly-deposited clays, marls, and sandstones, similar in all respects to those which all concur in marking as the upper part of the new red sandstone.

The evidences first, of a regular succession of strata on the sides of the hills; secondly, of the action of water at low levels; and thirdly, of similar structure of rocks in the lower as in the upper parts, denoting similar depth of water, lead to the conclusion that the land included in the Bristol district was, during the formation of such parts of the new red sandstone as are therein deposited, subjected to a gradual movement downwards, so that the waters first touched the lowest parts of the hills, and then gradually ascended up to the highest point at which the conglomerates are found. This hypothesis is confirmed by the following facts:—On the northern side of the Mendip hills, at the height of 750 to 800 feet above mean sea level, there is deposited a conglomerate of the age of the white lias resting on lias strata tranquilly deposited. On the tract of limestone northwards, called Broadfield Down, a conglomerate of similar age occurs at the height of about 550 feet. On the top of an isolated hill intermediate between these stations, at an elevation of 350 to 420 feet, occurs a lias conglomerate varying from 30 to 70 feet in thickness, not at the base of the lias, but likewise of the age of the white lias. This bed of conglomerate therefore descended from the shores on each side and crossed the valley at a lower level. The continuity of this bed renders highly probable the inference that the strata which are subjacent to this lias conglomerate on the hills, were also more or less continuous from shore to shore.

If these views be correct, if the order of succession presented by the strata accumulated on the slopes of the hills correspond with the order of time at which they were formed, then a means is afforded of approximating to the age of any given bed resting on the older rocks, by reference to some other bed of known age at a limited distance from the hills and at a lower elevation, with which the given bed may have been in continuity.

The dolomitic conglomerate containing the Saurians is situated about 300 feet above mean sea level. The nearest horizontal formation is the base of the lias, which is at nearly the same height. The deposits of similar age at a distance of nearly one mile, are lower by about 100 feet, and similar strata at two miles from the limestone range are depressed to the extent of 150 feet. Combining these facts with the principles previously indicated, the Saurians, which form the subject of inquiry, may be pronounced to have lived during the time of the latest parts of the new red sandstone.

Remarks in confirmation were made on the affinity of the Saurians with the Rhynchosaurs, and on the improbability that any part of the Permian system exists within the limits of the Bristol district.

Mr. H. E. Strickland exhibited some specimens of vegetable remains in the keuper sandstone of Longdon, Worcestershire, where they were first noticed by the Dean of Westminster. These are for the most part fragmentary and obscure, but some of them appear referable to the genus *Calamites*, and one specimen seems to be a *Foltzia*, a genus found in the new red sandstone of the continent, but only once before met with in Britain. [This was in magnesian limestone of Northumberland, see Lindley, Fossil Flora, plate 195.] The state of preservation of these remains is remarkable; for instead of being black and carbonaceous, as is usual with fossil plants of so great antiquity, they are of a light brown colour, and highly elastic, resembling recent dead leaves. When viewed under the microscope these vegetable fragments exhibit the cellular texture in great perfection. The only other locality in Great Britain where plants have been found in the keuper sandstone is at Ripple, three miles E. of Longdon, where *Calamites* occur, but the sandstone is not quarried there at present. The only animal remains found at these localities are small teeth and dorsal spines of the *Hybodus*.

Mr. S. Stutchbury exhibited a large cylindrical bone found by Mr. Thompson of Aberdeen in the "Bone-bed" of Aust Cliff on the Severn, and presented to the Bristol Institution. The strata at this spot consists of the insect limestone, landscape marble, and bone-bed of the lias, resting on the marls of the new red sandstone system. But since the fish remains in the bone-bed belong to the Triassic type, it may be equally well to compare any reptilian remains found in it with those of the new red sandstone. The present bone, though wanting both extremities, is two feet in length, and more than five inches in diameter at one end, where it is broken off abruptly: it is unlike any bone of Chelonian or Enaliosaur, but presents some resemblance to the long bones of small recent Batrachia, on which account Mr. Stutchbury considers it referable to the great Labyrinthodon of the new sandstone.

On the Cause of the general Presence of Phosphorus in Strata and in all fertile Soils; also on Pseudo-Coprolites, and the Conversion of the Contents of Sewers and Cesspools into Manure. By THE DEAN OF WESTMINSTER, F.R.S.

Since Liebig first suggested the application of fossil phosphates to the same purposes with recent bones and guano in agriculture, many inquiries have been directed to such localities as promised to afford a supply of bones, coprolites, &c.; the bone-bed of the lias, exposed on the shores of the Severn, has not yet been worked, and will not repay the cost of working, but the red crag of Felixstow on the coast of Suffolk has afforded many thousands of tons of phosphoric pebbles, mixed with bones of whales and elephants and other large mammalia, and with flint pebbles, siliceous sand and crag-shells; the phosphoric bodies show upon analysis a composition nearly identical with that of the true coprolite. The origin of the pseudo-coprolites in this remarkable deposit must be sought in a period antecedent to the crag, during which the London clay was in progress of formation, and when the muddy bed of the Eocene sea received daily accessions of phosphoric compounds from the dead bodies and fæces of fishes and Molluscs which inhabited it. The remains of these creatures, decomposing in the mud, evolved ingredients which, combining with the surrounding sediment, became fixed in Septaria and smaller concretions. In deposits of siliceous sand no such combinations could take place, and hence the barrenness of siliceous sands when converted into dry land. Phosphate of lime exists largely in all organized bodies, and is soluble slowly in water charged with carbonic acid: we may assume that all sea-water contains it; it exists in marine vegetables, and in herbivorous and carnivorous fishes and Molluscs. The combination of these phosphates with the earthy concretion not only purifies the water of the ocean and maintains it in a state adapted for the existence of living things; it serves also to form a continually increasing store of fertility against the time when the sea-beds shall be elevated and converted into corn-fields. While the crag was in progress, much of the London clay has been wasted by denudation, and its Septaria mixed with the shells and bones during the later period of the formation of the crag. It is probable that the Septaria absorbed a still further quantity of phosphoric matter during their accumulation in the crag: it is possible, also, that the peroxide of iron which pervades these pebbles and bones in the crag may have added to the phosphate when all the ingredients were in a semi-fluid state at the bottom of the sea. The Dean then referred to the discovery by Mr. Payne of beds of pseudo-coprolites in the upper greensand of Farnham. Here sponges and other organic bodies appear to have served as recipients of the phosphates; the Kimmeridge clay of Shotover Hill contains abundant casts of the air-chambers of Ammonites filled with marl, and containing 20 or 30 per cent. of phosphate of lime. Since all strata containing organic remains have more or less phosphoric compounds, these must also be present in the soils produced by their decomposition. Another large class of soils is produced from the decomposition of volcanic rocks and granite; in these phosphoric matter is also present, either combined with lime (apatite), or as phosphate of iron, and here its presence is unconnected with organized remains. In Spain the apatite forms an immense vein in ancient schists; and every specimen brought home by Dr. Daubeny has a radiated and stalactitic structure, showing that they were deposited from water, which must have taken it up previously from other rocks. In conclusion, it was suggested that, since clay and marl and lime are employed by Nature to absorb the phosphoric acid produced by the decomposition of

organized bodies at the bottom of ancient seas and lakes, so they might be applied artificially to deodorize and combine with the phosphates in the sewerage of large towns.

On an original broad Sheet of Granite, interstratified among Slates with Grit Beds, between Falmouth and Truro in Cornwall. By the REV. D. WILLIAMS, F.G.S.

This bed of granite is the only one of the kind ever seen by the author, who has traced it over a breadth of four miles by two. It varies in thickness from 4 feet 9 inches to 16 feet, and in dip from 15° to 40° ; in some places it undulates repeatedly with the slates, and in one there is a small shift in the slates, whilst the granite is only bent.

ZOOLOGY AND BOTANY.

On some Changes in the Male Flowers of Forty Days' Maize.
By ROBERT A. C. AUSTEN, F.R.S.

THE specimens I herewith send were taken from a crop of that variety of the *Zea Mais* which has recently been introduced into this country as the Forty Days' Maize: the seed was said to have been raised on the slopes of the Pyrenees, at an elevation of 3000 to 4000 feet, and the variety was considered as more likely to succeed than any of those as yet cultivated in this country.

As is well known, the *Zea Mais* is a monœcious grass: the male flowers are borne in distinct terminal panicles, which rise high and clear of the leaves; the female flowers are contained in lateral cobs, which consist of bracts enveloping a cone; these consist of several double rows (8–10) of flowers; of these the pistils project beyond the bracts. Several female flowers are grouped together; one only of each group usually perfects its seed, but the abortive ones can be detected, and help to account for the number of valves which are to be found in conjunction with each seed.

The external bracts serve to protect the whole cone of associated female flowers of the maize, and which are not therefore provided for by the hardening of the valves of the corolla, as in *Phalaris*, &c.; these envelopes are therefore but imperfectly represented in the ears of maize; and it will be observed that where the seed is abortive they are developed more fully.

Compared with a crop of four varieties of American maize, of which the heads of male flowers were all full and branched, the contrast was striking: a large proportion of the flowers of the forty days' maize were single like ears of wheat; another peculiarity was, that it presented a number of heads of naked grain; this change has been noticed, but instances may not have come within the observation of English botanists. Mr. Turpin, as quoted by Moquin-Tandon, thus describes it: "Where the transformation of stamens into pistils takes place, there is sometimes a single supernumerary ear, which is usually situated near the summit of the principal axis; sometimes several; in this case each branch bears its own." This is a true description of the appearance which the heads commonly exhibit; it is in the lower portion of the ear that the grain is wanting in wheat, particularly in cold situations or in cold seasons.

With the conversion of stamens into pistils in the terminal panicles, there is frequently a suppression of the lateral cobs.

The monœcious grasses are mostly tropical or sub-tropical; but in the present instance we seem to have an example of a hardy variety of *Zea* taking the character of the inflorescence of the grasses of the temperate and colder zones.

On a Series of Morphological Changes observed in Trifolium repens.
By ROBERT A. C. AUSTEN, F.R.S.

In a paper by Dr. Lankester to the Natural History Section of this Association last year, he referred to instances, he had recently observed, of proliferous clover. In

consequence of some remarks which fell from the several gentlemen named in that paper, at the time the specimens were observed, my attention was drawn to the subject: the results I here offer in the shape of a few notes to explain the drawings*.

To such as seek for illustrations of this branch of botanical inquiry, instances will rapidly accumulate from a large list of plants. Some genera however seem to present such changes more readily than others; and again, in some species of a genus they will be frequent, in others rarely, if ever, occur. Morphological changes are very common in *Trifolium repens*, occasional in *Tr. pratense*, but in this species they seldom extend beyond the calyx, whilst in *Tr. incarnatum* I have never yet detected an instance, though I have cultivated it for some years.

Mr. Babington, in his 'Manual,' after the description of *Tr. repens*, observes, "that in damp seasons the pod is often protruded in the form of a horn, or changed into a small leaf." This is an exact description of an appearance which the flower-heads frequently present, but the formations of pod or leaf seem to be exhibitions of contrary tendencies; in the one the plant hastens to accomplish its end, in the other it breaks away and reverts to the production of leaves.

About the end of May in this year, the flowerets consisted of a calyx of the usual size; the petals and stamens were rudimentary, and the plant, as if passing over two stages of its flower-structure, proceeded directly to the production of a pod. These pods contained ovule-like bodies, and were very much larger than the ordinary seed-pods of the plant.

About the beginning of June the clover-heads with enlarged pods had a very different appearance; one of the white petals was to be seen protruding beyond the calyx, and partly enclosing the pod; this petal was always the vexillum; the remaining parts of the flower being suppressed as before.

As the season advanced, the production of the "horn-like pod" was less frequent; and it was then principally that the substitution of leaves for flower-organs was to be observed: and at the present moment (Sept. 4) it would be difficult to find a single instance of change.

From this it would seem, that, according as the conditions at any particular moment may be favourable to vigorous growth or otherwise, the plant advances to the production of floral organs or reverts to leaves; and as the formation of the several parts of the flower follow in succession, from the calyx upwards, so the part of the flower exhibiting the change will be higher according as the plant's flowering-season has advanced.

The changes indicated in the drawings accompanying the paper were as follows:—

I. *Calyx*.—The calyx-teeth often rise into single leaves; but when compound leaves are formed, the division seems to be as follows: the two large equal teeth, which are opposite the vexillum, form one ternate leaf, and another leaf is formed from the three remaining teeth.

II. *Corolla*.—The part which here most frequently reverts to a leaf is the vexillum, and this a perfect one. Of these leaflets, the *alæ* are often seen forming simple leaves, as also the carina, but their perfect union into a ternate leaf is less common.

III. *Stamens*.—Whatever changes the flower may exhibit, these organs are always in a state to be recognised, and their reversion to leaves less frequent than in any other part; so that there is more difficulty in determining the number of leaves which go to form this portion. As two ternate leaves form the calyx and corolla, it might be supposed that the stamens were constructed out of the same number. The figures represent cases of a stamen reverting to a leaf with a true stamen attached to its stalk on either side; the single anterior stamen, when it reverts, seems always disposed to form more than a simple leaf, and it is therefore probable that the ten stamens (9+1) may be formed out of four sets of ternate leaves.

IV. From the well-known character of the pod and pistil in *Leguminosæ*, it might be expected that instances of reversion to leaf would be most frequent in this part of the flower; and a series might easily have been produced which would have represented it in every stage of passage; some of these were given. From these it would appear that the pod is not formed of a whole compound leaf, as either two scales, or two abortive leaves, are constantly to be seen at the base of the imperfect pod on either side; the pod is therefore usually formed out of the middle leaflet. In one

* The paper was accompanied by a series of drawings on an enlarged scale.

flower-head however each division of the pistil-leaf had become a pod, with a distinct stem, and the ovules inwards.

Ovules seem to be produced only when junction of the edges of the pistil-leaf takes place; in other cases leaflets are produced in the place of ovules.

In cases where every other part of the floral series has been regularly developed, the pistil occasionally will take the form of a perfect ternate leaf, and then the axis of the plant is continued through the flower.

Some of these changes have been already noticed and described; but one complete series, extending from the calyx through every part of the flower, has not, that I am aware, been recorded as to this, or indeed any other plant.

With respect to the leaves of *Tr. repens*, it is stated by M. Moquin-Tandon that they occasionally take additional leaflets, and he quotes instances of four, five and seven. In Link's 'Report on Botany' (Ray Soc. Translation), M. Walpers is quoted for a notice of "a monstrous seven-leaved leaf" of this species, who considers the three leaves, as well as the simple leaves, as shortened pinnated leaves.

From the very common occurrence of three simple leaves in the place of the compound one; from the instance already noticed of the termination of the axis in three opposite pistils, as well as from the structure of the base of the stalk of the ordinary leaves, it would seem rather that they consisted of unions of three simple ones. Though directed to look for instances of pinnated leaves by these notices of MM. Moquin-Tandon and Walpers, I was not able to meet with any.

On Fairy Rings, with Notes on some of the Edible Fungi by which they are caused. By Prof. BUCKMAN, F.G.S.

After detailing at some length the experiments of Mr. Way on the composition of fungi forming the fairy rings, Prof. Buckman gave an account of the various species which formed fairy rings in the neighbourhood of Cirencester. He stated that at different seasons of the year no less than three species of *Agaricus* appeared on the same ring. The species of Grasses also that composed the ring were found by the author to be constantly the same in the inner and outer parts of the circle in the rings which he examined. The Cirencester species of fungi in the rings were edible, and much sought after by the students of the college, being the *Agaricus prunulus*.

On a remarkable Monstrosity of a Vinca. By Prof. E. FORBES, F.R.S.

In this monstrous flower the calyx and petals were normal; the stamens converted into petals, with traces of anthers on the margin of their attenuated bases; within them were six carpels arranged in two whorls; the outer three had no styles and exhibited no sutures on their inner faces; the three inner ones were larger; two were sutured along their inner faces, two bore styles on their tips, the summits of the three styles had united by their basal rings; below the stigma, which was common to all three, two of the styles had been broken away in consequence of the growth of a prolongation of the axis from among the centre of the ovaries; this elongation bore upon its summit a rudimentary flower, consisting of five outer lanceolate segments equivalent to sepals, five linear bodies alternating with the former equivalent to petals; a five-lobed fleshy ring, which might be regarded as a circle of stamens, but which showed no traces of anthers; the four bodies equivalent to carpels, two of them larger than the other two, and one of the two bearing a style terminating in a stigma. The monstrosity did not end here; in the midst of these ovaries, arose another but very short prolongation of the axis bearing a cup-like disc, bordered by five leaf-like lobes, and within the margin of the cup was a circle of minute ovule-like bodies; all the parts of the prolonged axis were green. The observer was inclined to regard this singular monstrosity as an instance of true folial and true axile placentation co-existing in the same flower.

The monster was found among some flowers brought to Covent-garden this spring.

On the Varieties of the Wild Carrot. By Prof. E. FORBES, F.R.S.

Two species of *Daucus*, *D. carota* and *D. maritimus*, are enumerated as indigenous in our British Floras, and a third has been indicated with a doubt and referred to the

D. gingidium. The object of this communication was to show that the characters by which these supposed species were distinguished are by no means constant, but on the contrary extremely variable; that the *Daucus carota* passes gradually into the *Daucus maritimus* as it approaches the neighbourhood of the sea, and that the plant which has been referred to *Daucus gingidium* is also a sea-side variety of *Carota*. There is however an unnoticed form, probably of an extreme variation of the supposed *gingidium*, occurring on the coast of Dorsetshire, which is remarkable for having dusky yellow petals with ciliated margins, whereas all other forms of our carrots have white petals with entire margins. To this variety it is proposed to apply the name *ciliatus*. This plant, which at first sight has much the aspect of a veritable species, is probably the one mentioned by Decandolle as occurring near Dieppe, and referred by that author to *Daucus hispidus* of Desfontaines. It does not appear probable however that the plant so called by Algerine botanists is identical with that from the shores of the Atlantic, nor is there any sufficient evidence that either *D. gingidium*, *D. hispanicus*, or *D. littoralis* of Mediterranean floras have been found (as has been asserted) north of the Bay of Biscay. Living specimens of the plants described were exhibited to the Section.

On some Abnormal Forms of the Fruit of Brassica oleracea.

By EDWIN LANKESTER, M.D., F.R.S.

The specimens in which the monstrosities were observed were gathered from under the Culver Cliff in the Isle of Wight. In many of the specimens the fruit exhibited the external form of the silicle rather than the silique. The beak and the stigma, which normally are fully developed, were reduced to a mere rounded point, and in many cases the distance from the stigma to the pedicel was not more than the sixth of an inch. On opening these fruits no vestige of a septum could be found, and the partly developed ovules adhered on each side to a continuous mass of vascular tissue uniting the two carpels. Each carpel was broader than it was long, and was composed of a little leaf-like bag, which was puckered and contracted at its union with its fellow on the opposite side. Reticulated veins were easily observed on each of the metamorphosed carpels. From the fruits in this state up to those normally developed, were a series of transitional forms presenting almost every possible variety of form. The author suggested that these changes in the fruit of a cruciferous plant suggested the possibility that the septum, the beak, and stigma in the Cruciferae were not, as had been suggested by previous writers, foliar or carpellary structures, but that they had a true axile origin.

On the Vegetable Productions of Algiers. By G. MUNBY.

In this paper the author gave a sketch of the various plants which give a character to the vegetation of Algiers. He mentioned those which are used as the food of man. Amongst these he entered into a discussion of the species of plants which had been supposed to yield the *lotus* of the ancients. He also described the *Lichen esculentus*, a plant of rapid growth belonging probably to the order of Fungi, and which covers some of the desert wastes of Algeria. It has a sweet taste, is eaten by the Arabs, and is quite capable of sustaining animal life. Mr. Munby suggested that the manna recorded in Scripture might be a production of this kind.

On the Nervous System and certain other Points in the Anatomy of the Bryozoa.
By Prof. ALLMAN, M.D., M.R.I.A.

The first notice of a nervous system in the *Bryozoa* is due to M. Dumortier, who mentions the existence of a transparent body at the base of each of the tentaculiferous lobes in his genus *Lophopus*, established for the *Polype à Panache* of Trembley, these bodies being considered by Dumortier as true nervous ganglions.

In referring to the nervous system the appearance just mentioned, Dr. Allman was of opinion that this naturalist has fallen into an error; but it is nevertheless quite certain that Dumortier had observed the true nervous centre in a yellowish body which exists on the rectal aspect of the oesophagus just behind the mouth, and which he has referred doubtfully to the system of the nerves.

Without any knowledge of Dumortier's discovery, Professor Allman had demonstrated some years ago the existence of this organ in *Cristatella*, and had also referred it to the nervous system, describing it as the great œsophageal ganglion of the Bryozoon.

Professor Allman was now enabled to lay before the meeting some additional facts connected with this subject, as he had recently discovered filaments proceeding from the ganglion, so that the *distribution* of the nerves could now no longer be considered as a matter of doubt. The author described the great œsophageal ganglion in *Plumatella repens* as sending off a large filament to each of the tentaculiferous lobes, a smaller one passing off at each side to embrace the œsophagus, while a very short one appeared to proceed from the ganglion and dive into the substance of the œsophagus, where it could no longer be traced, and another set of filaments was observed to pass forwards and distribute themselves to the organs about the mouth.

Among other points in the anatomy of the *Bryozoa*, Professor Allman mentioned his detection of striæ in the muscles, and the tendency of the muscular fibre to break itself into discs. The tube of the tentaculæ was shown to be lined by a distinct membrane; the invaginated part of the internal tunic was proved to be composed of two portions distinct in structure, separated from one another by a sphincter, and a complete system of muscles was demonstrated in connexion with the oral valve.

From the facts now laid before the meeting, Professor Allman maintained the necessity of removing altogether the *Bryozoa* from the position among the radiate classes in which they had been placed by authors, and raising them at once to the sub-kingdom of the Mollusca.

On a New Freshwater Bryozoon. By Prof. ALLMAN, M.D., M.R.I.A.

The subject of this communication was discovered in the Commercial Docks on the Thames, during a late examination of that locality in company with Mr. Bowerbank. It possesses many points of resemblance with *Plumatella repens*, but differs essentially from this animal in the circumstance of each cell being separated from its neighbour by a distinct septum, as in *Paludicella*.

On the Reproductive System of Cordylophora lacustris, Allm.

By Prof. ALLMAN, M.D., M.R.I.A.

Certain branches of *Cordylophora lacustris*, instead of terminating in polypes, bear upon their extremities an oval vesicle, into which the contained matter of the stem is continued. These vesicles are filled with spherical bodies, and must be viewed as the true ovarian receptacles of the zoophyte.

On Lophopus crystallina, Dumortier. By Prof. ALLMAN, M.D., M.R.I.A.

In this communication the author noticed the occurrence of *Lophopus crystallina* in the pond of the Dublin Zoological Gardens. This elegant zoophyte is the *Polype à Panache* of Trembley, and had been first characterized as a genus by Dumortier. It has since been confounded with other genera, and Dr. Johnston, in his excellent 'History of British Zoophytes,' adduces Trembley's *Polype à Panache* as a synonyme of *Aleyonella stagnorum*. An examination however of the present zoophyte must convince us of its true generic distinctness, and of the correctness of the views maintained by M. Dumortier, and subsequently by M. Van Beneden, who has figured and described it in his memoir 'Sur les Polypes d'eau douce de Belgium.'

Mr. R. Ball exhibited a new dredge which he had recently constructed for natural history purposes, being an improvement on the instrument called Ball's dredge.

Mr. R. Ball exhibited a drawing, and described the structure of a specimen of *Bryarea scolopendra* found in Dublin Bay by Dr. Corrigan.

Notes on some Tubicolæ. By C. SPENCE BATE.

Terebella medusa.—The author remarked that while building, this annelide placed the material collected by its tentacular cirri upon its mouth, where it is, he presumes,

covered by the glutinous substance, which when dried forms the cement and tubing of the case. With its mouth the creature places the sand upon its back and then rolls itself from side to side, and again puts forth its tentacula in search of fresh material. The whole internal cavity of the worm in which the viscera exist is filled by a fluid, by which the animal has the power of moving, the loss of which entails destruction of all motive power; to preclude which circumstance, upon receiving any external wound, the animal will divide itself by contraction of the annular muscles anterior to the wound, which operation it will also perform in order to escape from the grasp of an enemy.

From the head of the animal to about the lower extremity of the stomach is a mass of white granular material, which the author presumes to be the ovaria, on either side of which are ducts leading into several pear-shaped sacs. Early in February the author noticed active motion of the fluid within passing in one direction, excited by a powerful set of cilia; shortly after some particles of the fluid existing within these sacs seemed to unite together, which became the earliest formation of a new creature; this little animal exists by the introduction within its own system of the parent fluid by which it is surrounded; this is done through a circular umbilical pulsating heart which opens by a slit, situated about the centre of the young creature. Shortly after, what the author has termed umbilical circulation ceases, and the young worm moves within the uterine sac; as the creature progresses the intestinal canal also becomes more perfect, and shortly after it leaves the sac and enters into a passage or oviduct, one of which on either side traverses the walls of the parent and opens into the rectum beyond the point where the intestinal tube is incorporated with the outer walls of the worm, and there voided.

Sabella alveolata (*Hermella*, Savigny).—After speaking of the habits of this annelide and the circulatory system, the author says, in relation to the organs of progression, besides all these there are other setæ situated upon the back; these perform a most important office in the œconomy of these creatures, which is to eject from the cell the faecal matter; that this may be accomplished the more easily, the intestinal canal is extended beyond the creature, making a sort of tail about one-fourth of the whole length of the animal, which is turned forward upon the dorsal surface; the expelled material is taken up by these delicate setæ and passed forwards from one to the next until it reaches the entrance to the abode, where, when under water, it is ejected with considerable force, but at other times it is deposited at the entrance and washed away by the first passing wave.

Notes on the Boring of Marine Animals. By C. SPENCE BATE.

The latest theory which, possessing novelty, has been advanced, is that of Mr. Hancock (Brit. Association, Swansea, 1848; and *Annals of Nat. Hist.* October 1848). Prof. Forbes at the same meeting stated, that "he endeavoured to find the crystalline spiculæ (with which the author affirms the foot to be armed for boring), but without success, either by the aid of the microscope or by chemical tests."

Throughout his paper Mr. Bate accepted the presence of these crystalline bodies as a thing proved, and endeavours to show that the holes dwelt in by marine animals could not owe their existence to any mechanical force by a creature so armed and formed, even supposing the rock sufficiently soft to be mechanically fretted away.

The author of this paper noticed a hole so deviating from the cylindrical in figure, that a prominent portion of the matrix projects so as to occupy a position between the anterior edge of the two valves. This fact he argues is in opposition to either of the three theories which naturalists have most favoured.

First. It is opposed to the theory of mechanical attrition by an armed foot, since the greatest protuberance exists in juxtaposition with the aperture in the mantle through which the foot must extend itself, and is thus shown to be inefficient for the purpose.

Secondly. To that advanced by Mr. Osler, that part which is nearest the foot and consequently the most liable to be acted upon, is the least so.

Thirdly. To that which presumes that the animal wears the rock by the means of its own shell, using it upon the principle of an auger; since the presence of such an irregularity precludes the possibility of either valve from moving ventrally forwards, and consequently from a rotatory motion.

Similar evidence may repeatedly be seen where *Pholas parva* bores into chalk: the depression between the posterior margin of the valves occupied by the hinge corresponds with a ridge in the matrix, a circumstance which it was impossible could have occurred if the animal had rotated.

Opposing thus all mechanical action, the author resumed the idea of a solvent, but was of opinion that the solvent should be looked for in the element in which the animals exist, and not in the resources of the animal itself. He presumed that it would be found in the presence of the *free carbonic acid held in solution by sea-water*; the œconomy of the boring animals being simple and uniform throughout creation, the solvent only being directed by them according to their habits, through the process of respiration and ciliary currents.

He next proceeded to show the action of sea-water upon limestone coasts, attributing their peculiar appearance to the presence of carbonic acid in the sea-water; and upon rolled limestone pebbles, particularly those which had been previously bored by small annelides, which he presumed gave a passage for sea-water to the centre of the stone, and tended to wear them rapidly and irregularly, evidence of which may be frequently seen in those which have been perforated by sponges, becoming half-buried in sand, where the exposed side is corroded, whilst the protected part remains uninjured.

The author's opinion was, that the boring of *Saxicava* was to be attributed to the same means, the animal only causing the solvent to act more uniformly. He believed that the perforations of *Saxicava* were the work of time, and his experience went to show that the animal could not bore deeper into the rock after it had lost the power of locomotion, which was very early in its existence; after which, excavation only continued to enlarge the cavity so as to adapt it to the increasing bulk of the animal, as well as the entrance to the excavation, which in the young barely exceeds the sixteenth of an inch, while in that of the adult it often equals the diameter of the animal, and always bore a corresponding ratio to the thickness of the shell; thickness of shell denoting age, and with years the diameter of the bore increases.

With regard to the *Pholas* tribe, he presumed that they penetrate soft clays through a modification of the same power; that is, in all the *Lithophagi* the calcareous rock is dissolved by the carbonic acid in the currents induced by the animal, but in that of the borers into clay, the wearing power is the mechanical action of those currents, which are greatly increased by the muscular power of the animal; the carbonic acid still taking up any calcareous matter which may be present, as in the case where they are found to bore in soft triassic sandstone. In this opinion he considered that he was supported by the observations of Mr. Osler, who in explaining how (what he presumes to be) the rasped waste is expelled during the process of excavation, says, "When the projected syphon is distended with water, the *Pholas* closes the orifices of the tubes and retracts them suddenly; the water which they contain is thus ejected forcibly from the opening of the mantle, and the jet is prolonged by the gradual closing of the valves to expel the water contained within the shell: the chamber occupied by the animal is thus completely cleansed." That the *Pholas* expels material by the force of currents, is shown in this passage, but it is only hypothetical that it had been previously rasped off by the shell. The foregoing evidence shows that *Pholas* cannot rotate within its cavity; consequently the waste, seen to be expelled by Mr. Osler, could not have been first rasped off; therefore it is not unfair to presume that it was worn off by the mechanical force of the current excited by the animal. It is only by presuming such to be the case that we can account how *Pholas candida* can be found to bore into clay and peat on the coast of Wales, pure sand at Exmouth, and lias at Lyme Regis.

The remarkable manner in which shells and rolled pebbles are perforated by *Cliona*, will easily, the author presumed, be explained by the same theory. He argued that a sporule of *Cliona* (which is a true sponge in all its conditions) first obtains a footing in some crevice, where it develops itself so as to penetrate the whole fabric, destroying the shell or pebble by simply fulfilling the condition of its existence, which is by pouring its currents in a given direction, until a passage be broken through by the corroding power of the carbonic acid in those currents. He mentioned a case which fell under his observation, in which a *Saxicava* was not only checked but turned aside and deformed by coming into contact with *Cliona*. Neither, from their own power, were capable of effecting a passage through the other. The sponge from its nature could not be acted upon by the solvent, which it is presumed the mollusk uses; neither could

Cliona (although using the same solvent) destroy the shell of the Saxicava, as both were capable of wearing the oyster-shell, because the Saxicava was protected by an epidermis,—a membrane which he presumed was given to mollusca in general for the specific purpose of defending the shell from the corroding action of the carbonic acid contained in the water in which they exist.

The author also mentioned an instance of two specimens of Saxicava found by him boring in the valve of an oyster, the one at right angles with the other, the one which met the other full on the side being flattened by the contact; this circumstance, together with the wound caused being still in apposition, clearly proves that neither has advanced or moved during a considerable portion of their existence; moreover a portion of the shell into which they have bored opposite to the opening of the mantle, as stated before, remains prominent, so as to stand between the anterior edges of the two valves, proving beyond a doubt the impossibility of the animal's capability of rotating upon its own axis.

Of the boring of Patella, the author argued that its form will preclude all idea of its boring by the action of its shell.

His observations upon the boring of the Buccinum into the shells of other mollusca attributed their power of perforation to the same source, that is to a current charged with carbonic acid passing through the buccal apparatus of that tribe, the lingual riband having no part in the operation; the portion of the incomplete perforation which would most correspond with this siliceous apparatus was left the most prominent part. The animal, he stated, takes about two days to perforate the shell of the common mussel, and performs the work without the least action on the part of the shell, as must be the case whenever a circular hole is bored by mechanical action.

The same theory he presumed will hold in the absorption of the columella in the family of the Purpuriferae; that which follows the long-continued residence of the Pagurus in the shell of Trachelipods: as also the groove sunk by Spirogyphus, which annelide affords a good example to illustrate the theory; for it not only sinks a groove in the shell on which it has erected its own, but should its contortions bring it "into contact with any portion of its own shell, it absorbs it equally with any other."

Upon the boring of Teredo he had nothing new to offer, never having had an opportunity of examining any but dead animals in old wood; he believed that their anatomy being so different from the mollusca which bore into clay and stone, would account for a different action on the part of the creature, except in Xylophaga, which being free, bored probably, as it is presumed, the Pholas bores; in this idea the author is, he argued, supported by the fact that this animal rarely bores more than an inch deep, and that only into saturated wood, invariably shunning the harder kinds.

The Prince of Canino made a few remarks on the characters which distinguish the little Blue Magpie of Spain (*Pica Cookii*) from that of Siberia (*Pica cyanea*, Pallas). He also stated that the new *Caprimulgus* of Hungary belonged to the genus *Cordylis*.

On the Genera of British Patellacea. By Prof. E. FORBES, F.R.S.

The great similarity existing between patelliform shells, the animals of which are so different that they cannot be included in the same genus, has long been known to naturalists, and is one of those apparent anomalies which have been laid stress upon as sources of uncertainty in palaeontological inductions: without however very good reason, for the remains of the mollusks in question are rarely found in the fossil state, and the great majority of fossils of that class of animals are such as can be confidently depended on. In the course of the researches undertaken by the author and Mr. Hanley for their joint work on the "History of British Mollusca," now in progress of publication, a fresh inquiry was required to be made into the propriety with which the British Patellacea had been assigned to known genera. It resulted that among our species we had two forms, for which it becomes necessary to construct new generic types, viz. the so-called *Lottia fulva* and *Lottia ancyloides*. Neither of these belong to *Acmæa*, with which *Lottia* is synonymous, but both differ essentially in characters of head, mantle, dentition, and in the latter case, position of body with respect to shell. As no established genus can receive them, for the former a new genus,

Pilidium, is proposed, to which *Patella cæca* of the 'Zoologia Danica' also belongs; and for the latter a new genus, *Propilidium*. *Pilidium* is allied to *Acmaea* on the one hand, and to *Propilidium* on the other, having the position in the shell of the former genus, and the tongue of the latter. *Propilidium* links *Acmaea* and its allies with *Puncturella* and *Emarginula*, like which it has the apex of the shell turned away from the head of the animal, but has a very different dentition.

The British *Patellacea* may be arranged as follows:—

1st Group. *PATELLIDÆ*. Cyclobranchiate animals; apex of the shell anteanal.

1. *Patella*. A. Branchial laminæ extending in front of head; branchial impression in shell unsymmetrical.

1. *Patella vulgata*.

2. *Patella athletica*.

B. (*Patina*). Branchial laminæ deficient in front of head; branchial impression subsymmetrical.

3. *Patella pellucida*.

2nd Group. *ACMÆADÆ*. Branchiæ cervical; apex of shell variable; rachis of tongue of comparatively single elements.

1. *Acmaea*. Transverse element of tongue double; tentacula oculiferous; apex of shell anteanal.

1. *Acmaea testudinalis*.

2. *Acmaea virginea*.

2. *Pilidium*. Transverse element of shell anteanal.

1. *Pilidium fulvum*.

3. *Propilidium*. Transverse element of tongue single; tentacula eyeless; apex of shell posteanal.

1. *Propilidium ancyloide*.

Then follow *Emarginula* (two species); *Puncturella* (one species); and *Fissurella* (one species); all members of a third group, linking the two former with *Haliotis* and *Trochus*. *Capulus* and *Calyptrea* are members of another family.

On Beroë Cucumis, and the Genera or Species of Ciliograda which have been founded upon it. By Prof. E. FORBES, F.R.S.

At the Birmingham Meeting of 1839, the author, in conjunction with Professor Goodsir, communicated an account of the British Ciliograda Medusæ. They then announced the existence in our seas of the true *Beroë Cucumis* of Otho Fabricius, which they had taken on the coasts of Zetland.

Since that time Prof. E. Forbes has availed himself of many opportunities for the observation of these animals, and has been successful in discovering some new features in their economy. He has taken the *Beroë Cucumis* in many parts of the coasts of England and Scotland, from the Zetland Isles to the Isle of Wight, and has not been able to find any sufficient differences among the individuals to warrant the recognition of more than one species. They vary greatly in size and colour; in the Hebrides they are not unfrequently taken 3 inches in length, but are usually very much smaller on the English shores. He has found that apparently at certain seasons numerous individuals of this *Beroë* produce in the line of their ciliary ribs, and from the belts of motor tissue at the base of the cilia, ovate egg-like pedunculated bodies of a bright orange colour. These can also be produced from the finer ciliary circles of the mouth and of the dorsal extremity. When the animal is in this state, any irritation near the ciliary ribs causes it to contract the neighbouring portion of the body over them, so as to protect them, sheathing the eggs as it were in deep membranous canals. Particular attention is directed to these gemmules or egg-like bodies, which may prove to be intermediate states of the *Beroë*. When the animal is in egg it is extremely irritable, and when irritated gives out the most brilliant vivid green phosphorescent light, always from the vessels beneath the ciliary ribs and from no other part.

The badness of the majority of delineations of this animal and a misconception of its true structure, have caused numerous false species and several genera to be constructed out of one. Thus in the 'Histoire des Acalephes,' by Lesson, all the following appear to be founded on *Beroë Cucumis*: in the genus *Beroë*, *Beroë Forskahlîi*,

Milne-Edwards, a name proposed for the *Medusa Beroë* of Forskall, which is the Mediterranean form of the species, and to which Professor Milne-Edwards, in an admirable memoir, very rightly assigned the *Beroë ovatus* of Lamouroux, the *Beroë elongatus* of Risso, the *Beroë rufescens* of Eschscholtz, the *Idya Forskalii*, *Beroë albens*, *Beroë Chiajii* of Lesson himself. The author suggested its probable identity with the *Beroë ovatus* of Brown, the *Beroë Cucumis* of Otho Fabricius and Sars, the *Beroë Capensis* and *Beroë punctata* of Chamisso, and the *Beroë macrostomus* of Peron and Lesueur. In spite of the elucidation of the subject by Milne-Edwards, several of the so-called species, as *Beroë albens*, *B. punctata*, *ovatus* and *Capensis*, were retained by Lesson and distributed even under different genera. *Beroë fallax*, founded on a figure by Scoresby, is probably the same species. In the genus *Idya* of Tremonville, retained on account of the animal having its "body open at the two poles" (a misconception founded on some of the curious contractile changes which these animals assume), we find *Idya Peronii*, which is *Beroë macrostomus* of Peron, *Idya Capensis*, *Idya Cucumis*, the names annexed to Fabricius's species, *Idya elongatus*, a six-ribbed monstrosity, mistaken for a species by Risso, *Idya borealis*, so far as the reference to Scoresby goes, and *Idya ovata* (*Beroë ovatus* of Brown), all almost without a question identical. Then follows the genus *Medea* of Eschscholtz (this to Lesson is a *Beroë* with interrupted bands of cilia), in which he places *Medea fulgens*, the species discovered by Macartney, and *Medea dubia*, founded on the fountain-fish of the old voyager Mertens, both undoubtedly *Beroë Cucumis*; whilst *Medea arctica*, founded on one of Scoresby's figures, was probably the same. The genus *Cyrdalisia* of Lesson himself follows, instituted on account of the presence of "two little ciliated openings" at the pole opposite the mouth. Every person who has examined a *Beroë* knows that the two little rays of cirri which Lesson means by this phrase, are present in every individual. The *Cyrdalisia punctata* is certainly *Beroë Cucumis*, and the *C. mitraformis* probably that species. Then comes the genus *Pandora* of Eschscholtz, which is defined on account of the ciliary bands being lodged in furrows bordered by membranous folds; evidently the condition of *Beroë* described in this communication. The *Janira hexagona*, founded on a figure of Slabber, was probably also the species before us.

Thus there would appear to be about fifteen species distributed under four if not six genera, constructed out of this one animal. Such proceedings tend to confuse zoological science, and are the more inexcusable since the full and accurate dissertation of Milne-Edwards on *Beroë Forskalii* was at hand to guide, and is even quoted in Lesson's work.

Prof. E. Forbes laid upon the table several papers containing observations made by the Dredging Committee. He hoped that the Committee would soon be able to present to the Section some general facts as the result of the investigations which had now been going on for so many years.

If Vitality be a Force having Correlations with the Forces, Chemical Affinities, Motion, Heat, Light, Electricity, Magnetism, Gravity, so ably shown by Professor Grove to be modifications of one and the same Force? By R. FOWLER, M.D., F.R.S.

The author, after having shown that each of these modified forces can be excited by any other, or in its turn be the exciter of all the rest, and consequently the antecedent or consequent indifferently of each of the others, proceeded to show that this is equally true of vitality, and that the coils in which these forces are latent, and by whose modifications in an excited state they are rendered apparent to our senses, constitute one of the differences between them. For instance, the change of temperature to which the infant is necessarily exposed at its birth, the heat going rapidly out of it, excites the motion necessary for inspiration. This gives the oxygen of the air access to the carbon of the blood by endosmosis; this again to animal heat. From that electricity may be obtained; and from electricity, by an appropriate coil, magnetism. Gravity the infant acquires by its growth, and can counteract by its muscular contractility. It may be said that an infant affords no evidence of the production of the forces, light, electricity and magnetism, but the experiments of Dr. Faraday have demonstrated that all these may be produced by the vitality of the Gymnotus, and rendered palpable

to our sight and feeling. So much for the qualities by which vitality has correlations with all other forces. But there still remains a difference—vitality is the artist of its own coils. No other force can make an organ of either an animal or a plant (the coil by means of which their vitality is evinced). Neither a Volta nor an Ærsted could have invented an eye or an ear, or even a graft by which the sap of a fruit-tree is so modified as to differ from that of the parent stock.

The author added instances of the light of fire-flies, glow-worms, and some marine animals, as instances of production of light, apparent to the vision of others by vitality. And any person may satisfy himself of the ease with which a flash of light, the products of his own vitality, may be rendered perceptible to himself, by putting a plate of zinc between the gums and the cheek in one side of the mouth and the broad handle of a silver spoon in the other, and then (in the dark) he will see a flash of light at every instant of contact and separation of the zinc and silver.

That mind and vitality reciprocally excite and depress each other must be obvious to all who are attentive to their daily feelings; and all conversant with surgical practice must be aware of the difference in healing of wounds in a healthy or exhausted subject.

J. G. Jeffreys, Esq. exhibited some rare mollusca which had been recently collected by Mr. Barlee in Zetland. Among these were *Rissoa eximia*, a new species, of which Mr. Jeffreys gave a description; *Diphyllidia lineata* (Otto), new to the British coasts; *Fusus Berniciensis*; *F. albus*; *Trochus formosus*; *Cerithium nitidum*; *Rostellaria pes Carbonis*; *Scissurella crispata* (which was taken by Mr. Barlee alive, and adhering to stones like *Emarginula*), *Megathyris cistellula* and *Tellina balaustina*.

On the Course of the Blood in the Circulation of the Human Fœtus in the Normal Developement, compared with the Acardian, Reptilian, and Ichthic Circulation. By Dr. MACDONALD.

On the External Antennæ of the Crustacean and Entomoid Class, and their Anatomical Relation and Function, showing their connexion with the Olfactory instead of the Auditory Apparatus, and the Homology in the Vertebrate Class. By Dr. MACDONALD.

On Lucernaria inauriculata. By Professor OWEN, M.D., F.R.S.

Professor Owen communicated a description of the external characters and anatomy of a *Lucernaria* which he had found near low-water mark on the flat rocks to the east of Dover, August 1849, attached to the *Ulva latissima*: it differed from the *Lucernaria quadricornis*, or *L. fascicularis*, in having the eight tentaculiferous lobes equidistant from each other, and it differed from the *Lucernaria auricula* in the absence of any ear-like appendage at the middle of the border of the connecting webs between those lobes. It differed from the *Lucernaria campanulata* in the absence of the "two series of foliaceous processes arranged on each side of a white line*," extending from the sides of the mouth along the middle of each connecting web; and by the presence of a convoluted coloured filamentary body extending from the circumference of the mouth to the tentaculiferous extremity of each of the eight lobes. It also differed from the *L. cyathiformis* in the tentacles being supported, in clusters, at the extremity of lobes produced beyond the margin of the infundibular disc.

The specimens varied from an inch to half an inch in length. One variety had ten lobes. The stem of the polype, which is ordinarily slender and as long as the expanded body, terminates in an adhesive disc or base, in the centre of which is a small triradiate pore or pit, with a thickened border or sphincter. There is no cartilaginous lamina in this disc. Four canals commence from the central pore or pit, and ascend the stem projecting into the central digestive cavity, but separated from the cavity by its lining membrane, which is reflected upon the four canals; forming as many longitudinal folds projecting into the digestive cavity.

* Johnstone's British Zoophytes, 1846, p. 249, fig. 56, b.

The four canals bifurcate, and are continued along each of the eight lobes to their extremities, where they subdivide, and are continued along each of the thirty or thirty-two tentacles forming the terminal cluster of the lobe. Professor Owen described a small triradiate pore at the round expanded end of each tentacle, and assigned reasons for regarding it as the orifice of the canal traversing the tentacle. He entered into a disquisition as to the function of this system of ramified canals, which is equally distinct from the digestive and generative systems, and contains a clear colourless liquid, with minute organic particles or granules; and expressed his opinion that the system of ramified canals was homologous with the partially-divided abdominal or aquiferous cavity from which the canals of the tentacula are continued in the *Actinæ*.

The generative organs were arranged in eight filamentary masses, disposed in short wavy folds along the inner surface of each of the eight lobes. Each mass consists of a central lobulated body containing the fusiform capsules of the spermatozoa, similar to, but smaller than those of the *Actinæ*, and this body is surrounded by the looser stroma containing the ova. The large mature and impregnated ova dehisce from the inner surface, or that next the cavity of the infundibular web of the polype.

Professor Owen having travelled from Dover to the meeting at Birmingham, had not had the opportunity of comparing his observations with those of any other author, except such as were given in Dr. Johnstone's excellent work on 'British Zoophytes,'—the best manual for the sea-side observer. In reply to some remarks respecting the four longitudinal muscles or ligaments, described by Sars and others as rising up within the pedicle, he remarked that nothing was easier or plainer than the demonstration of the area of four corresponding canals in his *Lucernaria*: it required only a neat transverse section of the stalk or peduncle to see that they were not solid, but hollow bodies. The pores on the clavate ends of the tentacles were best seen by viewing them as opake objects with a good reflected light.

Since making the above communication, the author has compared his species with all the extant descriptions and figures of *Lucernaria*, and finds so close a resemblance in that figured in the late French illustrated edition of Cuvier's 'Règne Animal,' as to lead him to conclude that it is the same species. It is referred in that work, however, to the *Lucernaria auriculata*, although differing, like the Dover specimens, in the absence of the ear-like appendages to the webs signified by the name. If those appendages be constant, the specimens described by Prof. Owen, as well as that figured in the 'Règne Animal,' are a distinct species, for which the name *Lucernaria inauriculata* is proposed.

On Improvements in Pathological Drawing. By JAMES PAXTON, M.D.

The intention of this communication is to recommend the style of *cartoon* painting as well-adapted to pathological representations. The author has shown that the pictorial features of pathological drawings ought not to be deficient in the beauties of fidelity of expression and execution, but should possess these qualities in common with works in the fine arts of a more inviting character. For this purpose he has introduced a corresponding mode of painting; namely, illustrations of morbid anatomy by *cartoons*, with *plate glass* to give the transparent lustre which belongs to oil paintings, examples of which were exhibited to the Section. The colours employed were opake and solid; permanent white being the medium for moderating the intensity of each tint, in lieu of the transparent medium commonly used on white paper. The lighter and brighter parts of the object are brought out by penciling an opake body of colour on a darker ground. By this method a person has a singular facility of copying the appearances displayed by disease. Thus a coloured sketch of *Scirrhus Pylorus* was executed in an hour. To produce as much of the character of disease on white paper with transparent media, would have occupied several hours. In a highly-finished drawing of *Bright's disease* of the kidney, it appeared the labour was vastly diminished by penciling the indications of granular fibrinous deposits upon a deeper colour.

The advantages of adopting this method of pathological drawing were pointed out as manifold. First. That we obtain a portraiture of practical medical anatomy in a comparatively short space of time, and this circumstance is an important consideration, since all dead animal substances speedily undergo a succession of changes in colour. Second. Cartoon painting is peculiarly suited to morbid subjects, inasmuch as it displays their surface, organic development and texture, with greater distinctness and force than

any other style the author is acquainted with. Moreover it is a style which most readily admits of being rectified wherever it is observed to be defective, as the drawing is uninjured by laying one colour over another to any extent thought needful. Third. Placing plate glass before the picture gives both the brilliancy and softness of varnish on the surface.

The last suggestion mentioned relative to pathological delineation is, that while the drawing is in progress, the morbid specimen should be kept under a glass dome or roof; the latter, of a prismatic form, was preferred, from its not reflecting surrounding objects. The escape of any infection or unpleasant effluvia is thus prevented. Besides which, this plan preserves the part from the dryness and corrugation which soon take place from exposure to the atmosphere.

On the Luminosity of the Sea on the Cornish Coasts. By C. W. PRACH.

The author described the state of the weather at the time of observation, comparing it with that which occurred soon after, as well as the animals observed on those occasions. He exhibited drawings of many—some new to the British coasts,—one at least of which has been found in the Mediterranean. These were abundant in July, but were destroyed by a heavy gale of wind, since which they have not been noticed; they belong to the Diphyidiæ. The author had a long list arranged in a tabular form of the animals, state of weather, date and hour of observation, the amount of luminosity, &c.; but only gave a list of animals observed, with a table of the number of observations made in five years, and the changes of weather that took place soon after.

VERY LUMINOUS.

When the weather has changed suddenly from fine to wet with gales of wind, and at times tempestuous, with lightning, &c.		When it continued fine.
1845.....	1	1
1846.....	1	
1847.....	9	2
1848.....	13	4
1849.....	16	3

LIST OF OBJECTS OBSERVED.

Gasteropoda.

Young of *Eolis*.

Tunicata.

Tadpole of *Botryllus*.

Cirrhopoda.

Young of— barnacles and cast skins of.

Crustacea.

Opossum Shrimp; *Zœa*; *Oniscus cæruleatus*; *Polyphemus*; *Cyclops*; *Cypris*.

Annelida.

A small swimming Annelid.

Zoophyta.

Laomedæa, &c.

Aculephæ.

Willsia stellata.

Saphenia dinema.

Sarsia prolifera.

Thaumantias octona.

————— *inconspicua.*

Bouganvillia nigritella.

Lizzia blondina.

Lizzia octopunctata.

A new one.

Several other objects, much like the young of *Zoophytes*.

BEROË.

Diphyidiæ, probably *Cuboides vitreus*, and one something like *Calpe pentagona*, both new to the British seas.

Observations and Experiments on the Noctiluca miliaris, the Animalcular Source of the Phosphorescence of the British Seas; together with a few general remarks on the phenomena of Vital Phosphorescence. By Dr. J. H. PRING.

The author referred to the various theories which have been advanced on the subject of the phosphorescence of the seas, and to the instances of phosphorescence occurring both amongst land and marine animals. After reciting various observations by others on the phenomena of vital phosphorescence, he proceeded to detail his own experiments at Weston-super-Mare, upon a small vesicular animal, not exceeding the one-thousandth part of an inch in diameter, which possessed very remarkable luminous properties. This animal the author believed to be the *Noctiluca miliaris*, and he regarded it as the most common source of the phosphorescence of the British seas. It occurred sometimes in such large quantities, and was so luminous, as to give the sea the appearance of a sheet of fire. Viewed by aid of the microscope, the animalcule is seen to consist of a spherical portion, and a tentaculum, which appears to be a motor organ. It does not appear to possess any special luminous apparatus, but the phosphorescent power is believed by the author to reside in a flocculent mucus secreted by the little animal. In giving the results of his experiments on the light emitted, the author found that galvanism produced no perceptible effect, but the electro-magnetic current sensibly increased the luminosity. Oxygen gas increased the light, without exerting any marked influence over the duration of the life of the animal. Contrary to what might have been expected, carbonic acid gas was likewise found to increase the light to a very remarkable degree, far exceeding in this respect the effects of oxygen; but the animal was killed by it when immersed in it only for a comparatively short space of time. Sulphuretted hydrogen speedily deprived the animal of life, and consequently destroyed the light; nitrogen, nitrous oxide, and hydrogen produced little or no effect on the luminosity; strong mineral acids increased for a moment but speedily afterwards destroyed the light; æther instantly destroyed the life of the animal; chloroform increased the light and then destroyed the animal. The author then instituted a comparison between his own experiments and those of Prof. Matteucci on the glowworm; and after examining the various theories put forward to account for the luminosity of animals, concluded that the phenomena could not at present be referred to any more general fact with which we are acquainted.

Notice of two additional bones of the Long-legged Dodo or Solitaire, brought from Mauritius. By H. E. STRICKLAND, M.A., F.G.S.

These bones have been recently sent to England by the officers of the Royal Society of Arts and Sciences of Mauritius. They consist of two tarso-metatarsal bones, of which one is incrustated with stalagmite, and seems to belong to the same individual as those figured in the 'Dodo and its Kindred,' plates xiii. xiv., which are now in the Paris Museum. The other specimen is far more perfect than any examples of this bone before known, but though apparently belonging to an adult individual, it is of small dimensions, being only 5 inches 8 lines in length. The only defective portion is the posterior surface of the ecto-calcaneal process, which is slightly abraded. The form of the bone precisely agrees with that of the Solitaire, and though of small size, it is doubtless identical in species with that bird. It exhibits in great perfection all those peculiar characters which prove both the Dodo and the Solitaire to have been closely allied to the family of Pigeons; especially the position of the calcaneal canal, which in those birds passes *externally* to the posterior ridge, whereas in the gallinaceous birds it passes on the *inside* of that ridge.

The author took this opportunity to allude to a paper on the Dodo, in the Boston Journal of Natural History, in which Dr. Cabot, though wholly unacquainted with the researches of Mr. Strickland and Dr. Melville, arrives independently at precisely the same conclusion, viz. that "the Dodo was a gigantic pigeon."

On the Growth of Silk in England. By Mrs. WHITBY.

I had proposed offering to the British Association a short account of my progress in the art of cultivating silk in England, but I left Newlands before all the produce 1849.

of this year could be wound off from the cocoon, and it will not therefore be in my power to make my report as full or as statistical as I could desire. I am however unwilling that this meeting should pass without endeavouring in some way to satisfy the expectations of those who have been sufficiently liberal to pay regard to my convictions, that the cultivation of silk may with little trouble or expense be made general, and in the end become a profitable speculation.

From the period when I had the honour to place before you an account of my early trials, I have paid attention to the cultivation of the mulberry, especially of that species which I introduced in 1846, viz. the *Morus multicaulis* of the Philippine Islands. I have three other kinds of white mulberry, which all grow well at Newlands, but as none are so easily propagated as the *Multicauli*, or bear so great a weight of leaf, I have increased my plantation with them chiefly.

I said in my letter to the Royal Agricultural Society in 1844, that it was as easy to do so as to propagate the willow. I now say that it is much easier, and the produce is more abundant. The produce of the leaf this year has been immense, and even now, after having plucked them closely to feed my silkworms, they are strong and vigorous, and present a luxuriance of growth scarcely to be credited unseen.

I find the cuttings, which are rooted in the open ground, produce stronger and healthier plants than those struck under glass.

One of my earliest pupils has a productive nursery at Godalming of the *Morus alba*; many others in different parts of England are planting; and if gentlemen in England and Ireland, who have a few acres or roods of land to spare, would plant mulberries for posterity as they do their oaks, we should in a few years be independent of other countries for our supply of raw silk.

With regard to the rearing of the silkworm: as their habits become more practically known to me, I find less difficulty in bringing them to perfection; and am confirmed in my belief, that with due attention to their peculiarities they may be reared in England as well as in any other country, and with as little loss by death. Equable warmth throughout the period of their existence (which may be shortened or prolonged at pleasure), cleanliness, classification and ventilation, with the adaptation of the food (as to its maturity) to the different ages of the insect, will ensure success.

I have been this season very successful in rearing the worms I was able to hatch; they had no disease of any kind; they made their cocoon in thirty days; and the silk I have been able to wind off is as strong and bright and beautiful as that which, in 1844 and 1845, was pronounced superior to the best Italian raw silk.

There are many persons in England, and a few in Ireland, who have begun the experiment on a small scale. It requires time to mature and perfect any undertaking; but if I live long enough, and the growth of the mulberry becomes generally encouraged, I have no doubt my ardent wish to see the cultivation of silk established in England will be realized.

ETHNOLOGY.

On some remarkable Primitive Monuments existing at or near Carnac (Britanny); and on the Discrimination of Races by their local and fixed Monuments. By Dr. BLAIR.

DR. BLAIR described a visit made in 1834, with Mr. Francis Ronalds, to the bourg of Carnac, in the department of the Morbihan,—the territory of the ancient Veneti,—on the south coast of Brittany, for the sake of examining certain very remarkable monuments of the kind usually held for druidical, but of peculiar character and unusual dimensions, and hitherto but slightly known in this country. He observed upon the surprising richness in these remains of the small district, hardly six miles east and west from Carnac, which they explored. Their minute description of upwards of seventy notable objects had been printed,—and accurate drawings of the more important lithographed,—but not yet published. These antiquities were of the

usual classes,—pillars, dolmens or cromlechs, tumuli, circles, &c. A stone of sacrifice was mentioned, hollowed for receiving the back and shoulders of the resupine human victim; an obelisk, now fallen and broken, measuring sixty-four feet in length, and computed to weigh upwards of 300 tons; and the Mont St. Michel, a large tumulus surmounting a natural hill, having on it a chapel dedicated to St. Michael, whence its pristine design and use as a temple were inferred. The chief objects of interest, however, were five instances of an arrangement hitherto not elsewhere found. Nine, eleven, thirteen parallel rows or lines of pitched stones, varying from a huge to an almost diminutive size, form so many *parallelitha*, traversing and featuring the country. The lengths, too, are various. One springing near the bourg of Erdeven, extends a mile and three furlongs. Adjoining the heads of the *parallelitha*, are inclosures, viewed as temples, to which these long avenues led. Three of sets of lines lie consecutively and suggest the impression that they were continuous. The remaining two lie apart from them, and from each other. If, as the Rev. John Bathurst Deane,—the precursor, and, by his excellent chart of the ground, (in the twenty-fifth volume of the ‘Archæologia,’) the main guide of these travellers,—has reasoned, the intervening spaces were once occupied by similar series, connecting the five into one enormous Dracontium or temple of serpent-worship, imitating the windings of the deified reptile, the whole monument must have been the most stupendous of its kind in the world. To this were compared certain Scandinavian monuments, having the same character of Fields of Stones; differing herein, that the stones are disposed for the marking out of promiscuous graves; some being set to express the figure of a boat. The Swedish antiquaries account them battle-fields, turned into the cemeteries of the slain; the boat indicating where some illustrious sea-king or sea-hero lies. Attention was invited to this agreement between the monuments of the old Scandinavians and their historically-known spirit and manners; whilst those of the Bretons represent, if aright understood by us, that domination of the great druidical hierarchy, which stands out as the most conspicuous feature in the social constitution of ancient Gaul. Subordinate discriminations of the same tendency were pointed out in certain Scandinavian circles, considered as courts of law, suitably to the jealous respect for law and the litigious temper of the old Norseman at home; whilst the sacerdotal character of the Celtic remains re-appears, in the Logan or Rocking stones, spread over the Celtic—unknown seemingly on the Germanic—soil, and supposed ministrant in oracular uses. The importance of thus identifying the characters and monuments of nations, was urged in an ethnological view. The extraordinary remains at Carnac invite alike the scientific and the traveller for mere pleasure.

On the Alphabet of the Indian Archipelago. By J. CRAUFURD, F.R.S.

The paper summed up by stating that the nine alphabets of the Archipelago are the produce of five large islands only out of the innumerable ones that compose it. The most fertile and civilized island of Java has produced the most perfect alphabet, and that which has acquired the widest diffusion. The entire great group of the Philippines has produced a single alphabet; even this one is less perfect than the alphabets of the western nations, in proportion as the Philippine islanders, when first seen by Europeans, were in a lower state of civilization than the advanced nations of the west of the Archipelago. * * * * The Indian islanders write on palm leaves, which have received no other preparation than that of being dried and cut in slips; on the inner bark of trees, a little polished only by rubbing; on slips of the bamboo-cane simply freed from its epidermis; and on stone, metal, and finally paper. The palm-leaf employed is that of the Contar, or *Lontarus flabelliformis*. * * * The instrument for writing with on the palm-leaf, on bark, and on the bamboo is an iron style; and the writing is in fact a rude engraving, which is rendered legible by rubbing powdered charcoal over the surface, that falls into the grooves, and is swept off the smooth surface. The Javanese alone understand the manufacture of a kind of paper. This is evidently a native art, and not borrowed from strangers;—as is plain from the material, the process, and the name. The plant, in the Javanese language, is called *gluza*, *Broussonetia papyrifera*, and the article itself *dálwwan* changed into *dálancan* for the polite language. The process is not the ingenious one of China, India, Persia, and Europe, but greatly resembles that of making the

Egyptian papyrus, and still more closely the preparation of the South Sea cloth, the raw material being exactly the same. With the exception of the Javanese, it does not seem that the natives of the Archipelago ever wrote with ink before they were instructed by the Arabs. Even paper is generally known to the Indian islanders by its Arabian name of *Kārtas*: so that it is probable that a true paper was imported long before the arrival of Europeans, although the natives were never taught the art of preparing it. At present European paper is in general use by all the more civilized nations, to the exclusion of Asiatic.

On the Oriental Words adopted in English. By J. CRAWFURD, F.R.S.

The following is a list, according to Mr. Crawford, of such words of Oriental languages as in comparatively modern times have found their way into our own tongue. The greater number will be found in Todd's edition of Johnson's Dictionary; and the rest, with few exceptions, in Webster's American Dictionary. The words that he has collected amount to 160, and came to us, he says—often indirectly, however,—from the Arabic, the Persian, the Turkish, the Hindú, the Malay, the Chinese, and the Polynesian tongues. Following this arrangement of languages, Mr. Crawford gave the list in alphabetical order for each class.

Words derived from the Arabic:—Admiral, Alchemist, Alchemy, Alcohol, Alcoran, Alcove, Alembic, Algebra, Alkali, Amber, Ambergris, Arab, Arabian, Arabesque, Arabic, Arrachi, Arack, Assassin, Barb, Cadi, Caliph, Chemistry, Civet, Chouse, Coffee, Coffin, Cotton, Damask, Damaskeen (damson), Dragoman, Faquaer, Gallant, Gallantry, Hegira, Hookah, Hur, Huri, Islam, Lemon, Lime, Mahomet, Mameluke, Minaret, Mohair, Moslem, Musselman, Mosque, Nabob, Nadir, Naphtha, Nard, Spikenard, Olibanum, Opium, Orange, Otto of Roses, Ottoman, Ryat, Salam, Saracene, Saracenic, Scullion, Sherbet, Shrub, Sofa, Soldan (sultan), Sophy, Tabour, Tam-hourine, Talisman, Tamarind. Examples of these derivations:—*Alcohol*. *Al kahala* means the sulphuret or common ore of antimony, used by the Arabian women to blacken the eyelashes. According to the Dictionary of the Spanish Academy, the alchemists were in the habit of distilling this mineral along with ardent spirit, believing that a highly concentrated spirit was the result; and hence the word alcohol, a corruption of *al kahala*.—*Alembic*. *Anlik*, a still, with the article *al* prefixed.—*Coffee*. Arabic, *kahwah*,—Turkish, *kahve*. The English word evidently comes direct from the Turkish. The coffee plant is a native of Abyssinia, and not of Arabia, for it was not known at Mecca until 1454, only forty years before the discovery of America. The true name of the plant is *ban*; and *kahwa*, or coffee means "wine," as a substitute for which the decoction was used, although the legality of the practice was long a subject of dispute by the Mohammedan doctors. From Arabia it spread to Egypt and Turkey, and from the last-named country was brought to England in 1650.

Before quitting the list of Arabic words, Mr. Crawford said it might be noticed that the Arabs had effected, although in a rude way, far more than the Greeks and Romans towards making the eastern and western worlds acquainted with each other and communicating arts and knowledge. These (until inspired by the fanaticism of a new religion) house-keeping barbarians pushed their religion, arms, arts, and trade within thirty years to the western confines of India, and in eighty-eight years to Spain. They pushed their commerce to China and the remotest islands of the Indian Ocean, which neither Greek nor Roman had ever reached. We owe to their fanaticism cotton, coffee, the sugar-cane and culture of sugar, paper, arithmetical notation, race-horses, the whole citron or orange tribe of fruits, and all the various products of distillation.

From the Persian and Turkish languages there are,—Bashaw, Can, Caravan, Caravansary, Dervise, Emerald, Fairie, Hindu, Hindustan, India, Indigo, Jackall, Janizary, Jasmine, Lac, Lacker, Mogul, Musk, Satrap, Scimitar, Sepoy, Seraglio, Shawl, Semindah, Senanah, Tartar, Turband, Turk. We take an example—*Sepoy*. Persian, *sapahi*, a soldier, from *sapah*, an army. We have two forms of this word in English. We write the word *sepoy* when applied to an Indian soldier, and *sapahi* when it applies to a disciplined Turkish soldier.

From the Indian and Hindu languages there are,—Araca, Avatar, Bamboo, Banian,

Betel, Bramin, Camphor, Caste, Chintz, Chop, Cooly, Cowrie, Cubeb, Curry, Crone, Gentoo, Lac, Madapollams, Masulipatam, Mullagatawney, Muslin, Palanquin, Raja, Rupee, Sandalwood, Sugar, Sutte, Talapoin, Teak, Toddy.

From the Malay are,—Babigroussa, Bankshall, Bantam, Bird of Paradise, Caddy, Cassiowary, Catecheu, Cockatoo, Compound, Creese, Gambir, Gambago, Godoron, Gutta-purchah, Japan, Junk, Loory, Mango, Mangostin, Musk, Orang-outang, Paddy, Pical, Prow, Ratan, Sago, Sapanwood, Shaddock, Tabil, Upas.—*Orang-outang*. Malay, *oran-utan*, literally man of the forest, but more correctly a rude or uncivilized man, a savage, a clown, a rustic. The accent, as in nearly all Malay words, is on the penultimate in both words, and not, as we make it, on the last syllable. The naturalists, taking the Bornean individual as the type, establish a class of monkeys under the name of Ourangs; but the propriety of the term is very questionable indeed, seeing that *orang* means a human being, and is translated by the Latin word *homo*. The name of orang-outang for any kind of monkey is unknown to the Malays, and the natives of Borneo call the animal *mias*.

From the Chinese are,—Bohea, Congou, Hyson, Mandarin, Nankin, Soy, Tea. The number of these is small, owing to the imperfect monosyllabic dialects of China, which do not, of course, find a ready way into our polysyllabic language. Nearly the whole foreign trade of China is carried on in a jargon of English.

From the Polynesian, Mr. Crawford finds but three words in general acceptance:—Kangaroo, Taboo, and Tattoo.—*Kangaroo*. This word has found a place in our dictionaries, and was certainly supposed to be an Australian word by Capt. Cook, who first used it and described the strange animal to which it is applied; yet no such term is to be found in any Australian language.

On Ethnical Orthography. By the Rev. A. J. ELLIS, B.A.

On the Ghá Nation of the Gold Coast of Africa. By the Rev. A. W. HANSON.

Herein were given in detail numerous practices and ceremonies closely resembling those of the Jews. It was considered that they had not been borrowed from the Mohammedans, and that they were not arbitrary.

On the Ethnology of New Caledonia. By A. K. ISBISTER.

The tribes are referable to three divisions. Of these, the most important are the Tacullis (or Carriers), the best known of the Athabascan races.

On certain Additions to the Ethnographical Philology of Africa.
By R. G. LATHAM, M.D.

On the Transition between the Tibetan and Indian Families in respect to Conformation. By R. G. LATHAM, M.D.

Drawing attention to the researches of Mr. Hodgson (of Nepaul) on the Kocch, Bodo, and Dhimal, also to those of Dr. Bird on certain affinities between the monosyllabic and Tamulian languages. The Garo and Chepang tribes are the most important for the study of the transition.

On the terms Gothi and Getæ. By R. G. LATHAM, M.D.

In objection to the doctrine lately defended by M. Grimm, that the Goths and Getæ were identical, Dr. R. G. Latham found no reason to believe that the Goths were so called until they reached the Getic country, and that the name arose then and there, not earlier or elsewhere. Just as the Germans of England called themselves North-humbrians and South-humbrians (the last portion of the name being taken from the country to which they came), so did the Ostro-Goths and the Visi-Goths. Reasons were given for disbelieving the Guttones and Gothini to be Germanic.

On Tumuli in Yorkshire. By JOHN PHILLIPS, F.R.S.

In this communication the author explained, by descriptions of certain parts of the old Brigantian territory, and notices of the contents of tumuli which had been opened therein, the kind of aid toward tracing the physical character of ancient inhabitants of Britain which researches into tumuli might be expected to yield.

By recent excavations into tumuli on the dry chalk wolds, skulls of British, Anglo-Saxon, and Danish periods had been discovered, and as far as they had been interpreted they seemed to confirm the opinion that essential differences existed between the crania of Celtic and Teutonic races. Authentic data on this subject have been rarely produced in Britain; but the search for them appears likely to add the valuable evidence of physical structure to the conclusions of philology.

*On a Finlandic Vocabulary. By Prof. RETZIUS.**On certain American, Celtic, Cimbric, Roman and Ancient British Skulls. By Prof. RETZIUS.*

This paper consisted in the application of the theory of Arndt, Rask and others, as to the general diffusion of a race akin to the Finns over the whole of Europe anterior to the immigration of the Indo-European tribes. The Celt, generally considered as the earliest inhabitant of the British Isles, has a skull remarkable for its diameter from front to back. Such, also, are the skulls found in barrows of secondary antiquity. In the most ancient, however, the skull has its chief development from side to side; the conformation of the aboriginal nations hypothetically allied to the Finn and Laplander.

STATISTICS.

*On the Application of Statistics to the Investigation of the Causes and Prevention of Cholera. By Prof. W. P. ALISON, M.D.**On Prussian Statistics. By Chevalier BUNSEN.*

THE author made a statistical statement of the proportions of the races in Prussia, and on the railroads and schools of that country. His information was chiefly derived from the 'Statistischen Tabellen des Preussischen Staats,' by M. Dieterici, the head of the Statistical Bureau at Berlin, and from private information supplied by the same gentleman. We give a few of the points of this communication. Prussia had, exclusive of Neuschâtel, 15,536,734 inhabitants. At the end of 1846, 16,112,938. It therefore ranks fifth as to population of the European States. From Dieterici's tables we find the population of the great States in 1843 was as follows:—

European Russia, with Poland.....	54,762,207
Austrian empire.....	35,877,864
France.....	34,230,178
England	26,991,517
Prussia	15,536,734

Prussian Germany contains 1,940,000 Slavonic inhabitants. There are in the world 42,000,000 who speak German. In the United States, 4,750,000 of Germans, or their immediate descendants, still speaking German. In Pennsylvania, 49 per cent are German speaking people. In Prussia a census is carefully taken every third year. In 1815 Prussia had 10,250,000 inhabitants. The increase of population from 1815 to 1849 cannot be less than 6,250,000; for according to the constant proportion of increase, the census of 1849 will give a total nearer to 17 millions than to 16 millions

and a half; this is equal to the population of Belgium and Denmark. This increase is greater than in any other part of the continent. Prussian statistics do not afford a confirmation of the theories of Malthus. Of the increase, 20 per cent. is from immigration, the emigration to America being deducted. The chief emigration has been from the neighbourhoods of Minden and Trèves. In 1815, Berlin contained 150,000 inhabitants; in 1848, 420,000; in 1849, 11,000 less. Of the population, 4,500,000 are inhabitants of towns. In Prussia, there are to every 100 males 103 females; in France 104½. More boys are born than girls. In the earlier periods of life, males are to females as 100 to 99. The standing army of Prussia is 137,000; men capable of bearing arms, 837,000. In 1843, of 4,800,000 women, 2,200,000 are unmarried, or rather without husbands, as widows are included. The average age of marriage for women is from 20 to 21; for men, 25 to 26. Protestants are to Roman Catholics as 5 to 3; Jews number 206,500. The conversions of Jews were from 100 to 150 a year; but since their disabilities were removed, the conversions have increased 50 per cent.

On the County of Warwick Asylum for Juvenile Offenders.

By C. HOLTE BRACEBRIDGE.

The paper stated that the asylum was established about thirty years ago upon a simple plan. A few acres of land were attached to the farm-house engaged for the asylum, but they were subsequently let off, as the soil was not adapted for cultivation by boys, and they were now simply instructed in shoemaking and tailoring. The boys had all committed offences for which they were tried at sessions or assizes, and the coming to the asylum was entirely voluntary on their part, nor was there any means of detaining them. The education given was of a very plain and simple kind, but had been rendered more valuable by the pastoral care of the clergyman of the parish, the Rev. Mr. Powell. The committee of management consists of county magistrates chosen at quarter sessions. The average proportion reformed had been during the last three years about 65 per cent., and the average cost had been 16*l.* 6*s.* 8*d.* per annum, although 46*l.* 17*s.* might be considered the price the benevolent have paid for each reform. [?] A comparison showed that the expense of punishing a criminal boy without reforming him cost the country more than it did to reform him, amounting as it did to 18*l.* 16*s.* 10*d.* per head, exclusive of expenses defrayed by Government in the prosecution and in the transportation to penal settlements.

On the Fluctuations of the Annual Supply and average Price of Corn, in France, during the last seventy years, with particular reference to the four periods ending in 1792, 1814, 1830, and 1848. By J. T. DANSON.

It appeared from official sources that there are few of the departments of France in which the average consumption of grain of all kinds per head, per annum, falls short of an imperial quarter; that considerably more than half (by measure) of all the grain food thus consumed consists of wheat; and that though the use of wheat as a large proportion of the food of the people is confined to particular localities, these localities are so distributed that whatever changes materially affect them may be safely assumed to affect, more or less, the whole country. Hence it was inferred, in the first instance, that the official average prices of wheat might be safely accepted, in France as in England, as indicating the current price of food. The first period of sixteen years (1778 to 1793) was distinguished from every subsequent period of similar length, and from most of those preceding, by the low average range and also by the uniformity of its prices. The ten years' average of 1766 to 1775 was 18*l.* 66*c.* per hectolitre. From 1778 to 1787 it was only 14*l.* 33*c.* In any subsequent period of ten years the average had been very little over or under 20*l.* The average of the six years, 1788 to 1793, was 21*l.* 81*c.*; and during this period the most distressing fluctuations occurred; the average price of 1789 being more than fifty per cent. above that of 1787, and the price of 1793 (35*l.*) being more than a hundred per cent. above that of 1791. Thus ten years of low prices were followed by six of high prices; and these closed the period. The second period embraced the prices of eighteen years (1797 to 1814). From 1797 to 1802 prices were generally high in France, as they

were throughout Europe. But in the eight years, 1803 to 1810, prices were constantly low in France, giving a general average of only 18*f.* 60*c.*; and in each of these years, but especially the last seven, grain was more or less largely exported.

The averages here stated are, as usual, those of the astronomical, not of the agricultural years. They may therefore be taken to indicate the market value of about two-thirds of one crop and one-third of the next. So the first of the seven good years is to be referred to the harvest of 1803—of that summer during which Bonaparte formed the camp at Boulogne, and prepared his election to the Imperial throne in the following spring. The last ended with the gathering of the deficient harvest of 1810—the year in which the events of the Peninsular war began to run decidedly against France, and in which Napoleon determined upon urging his final and fatal dispute with Russia..

The common average of the three years 1811, 1812, 1813 was 27*f.* 66*c.*—an advance of more than fifty per cent. upon the average of the preceding eight years; and the whole rise of price, from 1809 to 1812, was from 15*f.* to 34*f.* per hectolitre. The harvest of 1813 was good, and after it was gathered prices fell; but this period was closed virtually by the battle of Leipsic in October, and formally by the abdication of the following April. The third period embraces the sixteen years from 1815 to 1830. It has a striking resemblance to the one preceding. The years 1816, 1817, 1818 were years of general scarcity, like those of 1800, 1801, 1802. Then also there was a middle period of plenty, marked by the nine years, 1816 to 1827, of moderate or low prices. The lowest prices, as before, were in the last years of this time of plenty; and they were succeeded by the scarcity and high prices of 1828, 1829, 1830. The price of bread in Paris was actually higher in 1829 than at any time in 1816, 1817 or 1818, or at any time since 1800. During the fourth period (the seventeen years from 1831 to 1847 inclusive) were nearly repeated the features which had distinguished the three preceding. At its commencement were two years of high prices. Then followed thirteen years (1833 to 1845), during which the general average of 20*f.* was only once materially exceeded, when, in 1839, the annual average rose to 22*f.* 49*c.* The common average of this period was 18*f.* 43*c.* But the period ended precisely as its predecessors had ended; with two years of prices, which, notwithstanding the use of foreign supplies, more than twice as large as had ever been imported in a similar period, were unusually high. Mr. Danson then went over the same ground again with another test—that afforded by a statement, from the Customs account, of the quantity of grain and flour of every description actually exported (of French produce), or exported and taken into consumption (of foreign produce), in each year; and exhibited, by tables and diagrams, a remarkable coincidence of the results obtained by the two methods. And finally, applying both tests conjointly to the period of ten years, 1838 to 1847, he showed the probable value of the excesses of exports and imports respectively, as indicating the addition to, or drawn upon, the national resources consequent upon the annual superabundance or deficiency of the home supply. The estimated value of the excess of imports in 1847 exceeded 320,000,000*f.*; and they were sufficient, according to the best authorities, to feed the whole population with grain food for forty-five days. In conclusion, two inferences were suggested:—1. That the political dates, 1792, 1814, 1830, and 1848, are also the *natural* divisions of a history of the French Corn Market since 1778; and 2. That the history of prices (especially as it regards the food of the people) might, in the order of practical importance to mankind, take precedence of the history of politics.

On the Progress of Emigration from the United Kingdom during the last Thirty Years relatively to the Growth of the Population. By J. T. DANSON.

The first complete census of the three kingdoms in 1821 gave us the total population 21,193,000; in 1831 the number returned was 24,306,000, showing an increase of 3,113,000 in ten years. Whether the number added in each year of this period was greater or less than the number added in the year preceding, could not be known from any comparison of these returns. But in 1841 the number returned was 26,916,000, showing an increase of only 2,610,000. It may therefore be presumed that the number added to the population in each year is now less than was added in the year before; but further, against this decreasing increment of the population, we

have of late years to place a rapid increase of emigration. During the ten years 1821 to 1831, the total number of emigrants was 738,582; and in the seven years 1842 to 1848 inclusive, the number was no less than 985,953. And according to the latest complete returns obtained by the Emigration Commissioners (down to the 20th of June last), the number of emigrants in the first half of 1849, was no less than 196,973. Hence it appeared that the emigration from the United Kingdom during the last three years was fully equal to, if it did not exceed, the natural increase of the population; and, in short, that emigration has now been carried on to such an extent, as, if it were maintained, must effectually prevent the further growth of the population.

On the Diseases and Causes of Disability for Military Service in the Indian Army. By C. FINCH, M.D.

The native soldier is not subject to a variety of diseases incidental to the European, and many of the complaints common to both are less severe, less complicated, and less fatal in the Indian, from physical constitution, simple nature of his food, and regular and temperate habits. He is in a great degree exempt from many of the acute, febrile, and inflammatory disorders so fatal to the European within the tropics. There is a marked difference in the character of the complaints common to both—those of the native being of the asthenic, and those of the European being of the sthenic diathesis. In the Asiatic, though there is a less tendency to a rapid course, there is less vigour to resist the encroachment of disease, which exhibits a proneness to become chronic and inveterate, occasionally terminating more tardily, but not less certainly, in death. This inferior power of the constitution renders them more prone to disease under slight exciting causes, less able to bear active depletory measures, or on the invasion of disease in an acute form they sink so rapidly that the mortality in the two classes is in a great degree equalized. From this inferior power of rallying from the attacks of disease, many disorders which at their commencement are acute become in their progress chronic, and terminate in rendering the native soldier incapable for service. In order to ascertain the particular diseases which incapacitate the Sepoy, and their relative frequency, it may be deemed requisite that an examination should be made, on a comprehensive scale, of the invaliding rolls, which besides containing a statement of the numbers, enumerate the diseases and causes of disability. On inquiry, Dr. Finch finds no such documents are procurable in this country; hitherto mere numerical returns have been made to the India House. In the absence of more extensive returns, he has been obliged to satisfy himself with the results of a more limited examination, but which will afford a close approximation to a knowledge of the real causes of disability, and enable us to form an estimate of their relative frequency. He has submitted to examination the invaliding rolls of three regiments for a period of nine years—from 1834 to 1842. These rolls are of the men presented for examination belonging to the 31st, 40th, and 57th regiments of Native Infantry on the Bengal establishment. Undoubtedly there are many circumstances which have a temporary as well as a permanent influence on the health of corps. The chief of these are climate, locality, and nature of the duties required of the men. By a fortunate coincidence, arising from a diversity in the course of their service during these nine years, he is enabled to give illustrations of the effects of these agencies. The 31st had lately returned from service in Afghanistan. The 40th had, within the period selected, completed three years' service to the eastward, at Kyak Phyon, and the 57th had returned from Barrackpore, after a triennial residence at that unhealthy station. By a reference to the general table exhibiting the numbers invalided, we find the following results:—That there were invalided from susceptibility to fever 5, and general debility, a frequent consequence of fever, 24; enlargement of the spleen, induced by fever, 3; or from fever and its consequences, a total of 32. That there were transferred from these regiments for rheumatism and contractions of the joints, a common consequence of rheumatic disease, no fewer than 68. That there had been incapacitated by asthma not fewer than 28; by dyspnoea not less than 6; and by consumption, 1;—from pulmonic disease altogether, 35. Disqualified by diseases of the eye, cataract, ophthalmia, and amaurosis, 14. Invalided

from diseases of the brain, apoplexy, mania, paralysis, and epilepsy, 13. There have been rendered non-effective by bowel complaints, by diarrhœa, 3; by dysentery, 5; total 8. Diseases affecting the whole system, such as scrofula, leprosy, syphilis, and cancer, 9. Eight have been removed from disabilities affecting the bones; 3 from fractures; and a similar number from loss of teeth. Exostosis 1, and periostitis 1. Incapacitated by diseases of the extremities, 13; 5 by wounds, of which 2 were received on service; by ulcers, 1; and by a peculiar disease, called "burning in the feet," 6. From cutaneous affections, 3. The other causes of disqualification, either accidental or anomalous, such as hernia, hæmorrhoids, &c., 25. During the nine years stated, 54 men have been incapacitated by general infirmity, or having become unfit—in fact, "worn out." An abstract of the general tabular statement shows, as disqualified for further service, 282—equivalent to 91 from each corps in this period, or 10 annually; and taking the strength of a native regiment to be 800, we have a rate of $1\frac{1}{4}$ per cent. It appears by the list that 54 men have retired from being worn out in the space of nine years. This class included all men who, by reason of their age or length of service, have become unfit for further duty. It is worthy of inquiry to ascertain what are the average periods of life and service at which the native soldier becomes incapable of further duty. In following up this inquiry, it will be necessary to ascertain the averages of ages and service of the several grades separately; for were the ages and periods of service of all ranks to be taken collectively in forming an estimate of the mean age or service of those who have been declared "worn out," it would be by no means a just one. Some of the native commissioned officers serve upwards of forty years, and are beyond sixty years of age at the period of their transfer to the invalids. By including their ages and periods of service in striking an average, we should obtain one obviously too high. It is therefore necessary to subdivide the men invalided, from having been worn out, into three classes, according to the several grades they were in when transferred, viz. commissioned officers, non-commissioned officers, and privates. The higher rate of pay, the lighter duties, and the superior pension, induce the native commissioned officers to hang on for a longer period than they would have done had they been in the inferior grades, and even to require a little gentle persuasion to present themselves to the invaliding commissioners when no longer fit for duty. The same advantages, though in a lesser degree, may have their influence on the minds of the non-commissioned, and induce them to continue in the ranks a few years longer, though, as far as regards pension or duty, there is no difference between the grades of non-commissioned officers, the Naicks and Havildars. Not having attained to these benefits, the Sepoy or private has no great inducement to remain longer in the service than he possibly can; and if he sees no immediate chance of promotion to the next grade, is anxious to exchange the active duties of his condition for the ease and comfort of retirement, and accordingly exaggerates his disability. These considerations have their weight, and may account for the comparative numbers of the several ranks, and the relative ages and periods of service of those who are annually removed. These remarks do not apply exclusively to those who are declared "worn out," but to all pensioned, whatever may have been their disqualifying causes for military duty. There have been fourteen commissioned officers pensioned in the period we have submitted for examination; their average age is 58 years 2 months, and their length of service 38 years 3 months. Twenty-five non-commissioned officers have been transferred in the same time to the invalid establishment from these three corps, of whom the average age is 47 years 8 months, and their length of service 26 years 6 months. During the nine years 11 Sepoys have been struck off the strength of these three corps, having been declared "worn out" after a service of 24 years 8 months, and at an average age of 44 years 8 months,

TABLE exhibiting the Diseases and Causes of Disability of the Men Invalided from the 31st, 40th, and 57th Regiments N.I., during the Nine Years from 1834 to 1842 inclusive.

Classes.	Specific Diseases.	31st.	40th.	57th.	Total.	Classes.	Specific Diseases.	31st.	40th.	57th.	Total.
Diseases of the brain—						Brought forward		54	60	43	157
Paralysis	1	2	2	5		Of the bones—					
Epilepsy	3	...	3	6		Exostosis	1	1
Mania	2	2		Fractures	1	2	3
Of the eye—						Periostitis	1	...	1
Cataract	2	2	...	4		Toothless	3	...	3
Ophthalmia	1	1		Of the skin—					
Vision impaired	3	6	9		Anomalous	1	1
Of the ear—						Ichthyosis	1	1
Deafness	1	1		Leper vulgaris	1	...	1
Of the chest—						Of the system—					
Asthma	13	9	6	28		Scrofula	1	1	...	2
Dyspnœa	1	5	...	6		Leprosy	6	...	6
Phthisis	1	1		Syphilis	1	1
Heart enlargement	1	...	1		Cancer	1	1
Of the abdomen—						Other diseases—					
Diarrhœa	2	...	1	3		Accidents	4	1	5
Dysentery	3	2	...	5		Anomalous	1	...	3	...	4
Spleen enlargement	1	2	...	3		Debility	11	2	11	...	24
Of joints—						Defective speech...	...	1	1
Contraction	1	...	2	3		Doubled hand	1	...	1
Dislocation	1	...	1		Fevers	5	5
Rheumatism	21	30	14	65		Hæmorrhoids	1	1	1	...	3
Of the extremities—						Hernia	3	3
Ulcers	1	1		Hydrocele	1	1
Wounds	2	2	1	5		Malingency	1	1
Varices	1	1		Testicle swelled	2	2
Burning in feet	1	5	6		Worn out	27	11	16	...	54
Carried over...		54	60	43	157	General total...		103	92	87	282

Annexed to the paper are the 'Vital Statistics of the East India Company's Armies in India,' by Col. Sykes; and in vol. iii. of our Transactions are tabular statements of the transfers and casualties on the invalid pension establishment; of the average length of service before transfer; average age at period of disease, and number of years each grade remained on the pension list for the years 1843 and 1844–45 for Bengal, and for Madras for the years 1842–43 and 1843–44. These statements include the whole native force of the Bengal and Madras Presidencies during the years specified, and offer, on an extensive scale, an opportunity of comparing the average periods of service and ages therein stated with those of the men invalided in the three native corps whose rolls have been submitted to examination. It will be seen that there is a close approximation in the averages stated at which the men of these regiments were transferred to the invalid establishment, and the periods mentioned in the tabular statements referred to. The average period of service before transfer of the two grades of commissioned officers in the Bengal Presidency was, in 1843–44, 38 years 2 months 5 days, and in 1844–45, 38 years 10 days. The average age of the commissioned officers was 56 years 4 months in 1842–43, and 51 years in 1844–45. The average age of non-commissioned officers was 47 years 6 months in 1843–44, and period of service before transfer, 27 years 9 months 18 days. The average period of the native soldier, or Sepoy, previous to transfer, was 16 years 10 months 4 days; and his age at date of transfer was 42 and 41 years respectively, for the years 1843–44 and 1844–45.

On a Form of Table for Collecting Returns of Prices in Ireland.

By Prof. W. N. HANCOCK, LL.D., M.R.I.A.

The primary object of the table is to direct attention to the observation of the facts which give the most correct indications of the state of the poorer classes. The statistical investigations which have been hitherto instituted into the condition of the population have been too much directed to quantities, whilst the more important observations of values have been neglected. Thus we have the census taken in Ireland in the most elaborate manner, showing, not only the number of the population, but the number of the trees, the number of the cattle, and even of the poultry, in the country. Then we have returns showing the sizes of farms; and the agricultural returns, showing the number of acres under cultivation for different kinds of crops. Now, I do not propose to undervalue these investigations; but as long as these returns are not accompanied by returns of prices, the partial knowledge deduced from them is likely to mislead. Thus, the most mistaken propositions have been stated as to over-population, from considering the population tables without reference to the rate of wages. Specious theories have with equal boldness been put forward as to the size of farms, from considering the land returns without any reference to the rent obtained from farms of different sizes. As to capital, again, we have had the boldest assertions respecting its want or abundance, arising from a consideration of the quantity of money in circulation, the deposits in the savings' banks, or some other quantity of capital, without any reference to the rate of profit. In like manner we have been told that there is no hope for a nation which lives on potatoes, or that the salvation of Ireland depends on the introduction of green crops, or of flax, without any scientific investigation of the average prices of such crops, or of the rent which they will produce. But economic science teaches us the real facts from which the condition of a population can be ascertained, and the advantage of different systems of management compared; and this table is constructed for the purpose of having these facts observed. If we want to compare the condition of the labourer in Connaught with that of the labourer in Ulster, in Scotland, in England, or in America, what do we require to know? Why, two sets of facts. First, what are the money wages or price of labour in Connaught as compared with the money wages or price of labour in one of the other places. Secondly, what are the prices of the commodities consumed by the labourer in Connaught as compared with the prices of the same commodities in other places? From these we can at once determine the relative condition of labourers at different places at the same time; and by similar investigations we can compare the condition of the labouring classes at different times in the same place. Thus, if we ascertain that the average price of agricultural labour is in Connaught 6*d.*, in Ulster 10*d.*, in Scotland 1*s.* 4*d.*, in England 1*s.* 8*d.*, and in America 3*s.*, whilst the prices of the commodities consumed by the labourer do not, on an average, rise in the same proportion, we see at once that the labourers in America are better off than those in England, who are again better off than those in Scotland, whilst the Scotch are better off than the Ulstermen, and they than the Connaughtmen. But the observations for which the table is constructed would, if systematically pursued, serve a scientific purpose of far greater importance. They would enable us to perfect the principles of economic science, and place them on a firm and lasting basis, by applying to them, more extensively and systematically than has been hitherto done, the inductive method of reasoning which has led to such wonderful results in the natural sciences; for observations of changes in values and prices are to the economist what observations of the movements of the heavenly bodies are to the astronomer—at once the facts to be explained, and the facts by which the truth or falsehood of his theories can be tested. The following is the form of the table:—It is headed with the name of the place where the observations are made; as the prices of different articles of wealth vary from place to place, it is necessary in all observations of prices, to note the place where the observations are made. Then, the prices to be observed are divided into six classes:—1. The price of labour, or rate of wages; 2. The price of the use of capital, or rate of profit; 3. The price of the use of land, or land-rent; 4. The price of food; 5. The price of fuel; 6. The price of other agricultural produce. Under each of these heads a sufficient number of kinds of each class are selected to be observed. Thus, under the first class we have—1. Agricultural labourers; 2. Weavers; 3. Carpenters; 4. Smiths; 5. Tailors;

6. Men servants; 7. Women employed in agriculture; 8. Sempstresses; 9. Spinners; 10. Women servants. In the second class, price of the use of capital, or rate of profit, we have—11. Interest charged by money-lenders to the poor; 12. Interest charged on bills above 20*l.*; 13. Cost of erecting a single-roomed cabin; 14. Rent of a single-roomed cabin. In the third class we have—15. Land let to yearly tenant; 16. Pasture-land; 17. Building-ground; 18. Conacre; 19. Tenant-right or good-will. For the fourth class we have—20. Flour; 21. Oatmeal; 22. Indian meal; 23. Potatoes; 24. Turnips; 25. Mutton; 26. Pork; 27. Fowls; 28. Eggs. For the fifth class—29. Coals; 30. Turf. For the sixth class—31. Flax; 32. Hay. Then, for the price of each article, there are two prices to be observed, viz. what is ordinarily considered a high price, and what is ordinarily considered a low price; and the table is constructed to have these observations kept for a period of six months, the prices being observed once a month. These tables are now in the hands of parties in Ireland, who are making observations.

On the Use to be made of the Ordnance Survey in the Registration of Judgments and Deeds in Ireland. By Prof. W. N. HANCOCK, LL.D., M.R.I.A.

He said that the Ordnance Survey at present was used to ascertain boundaries for all public purposes, and was also extensively used by private parties. The townland maps were complete for all Ireland, on which the name of every townland was marked with the number of acres contained in it. From the structure of the maps they afforded the greatest facility, for having tenements marked on them as well as townlands; as all the boundaries of tenements were on the sheets, except in some of the northern counties, the survey of which was first published, and on which the boundaries were now being inserted. On the brass plates from which the maps were taken, all these lines were laid down. He proceeded to show that the Ordnance Survey was thus adapted to be made the basis of a general registry of land, by requiring every deed or judgment to contain the names of the townlands and number of tenements affected by it. The reason which hitherto prevented the adoption in England of a plan of registry with reference to a general map, was the expense. As that expense had been incurred in Ireland, there was no reason why the maps should not be used. He proceeded to explain how the Survey might be used in registering judgments in Ireland; and said that the effect of the adoption of his suggestions would be to get rid of the delay and cost of negative searches for judgments and for deeds, which must now be incurred on every sale of every portion of land however small. In every case where the seller had a common name, or sold a number of portions of land, the delay and cost were at present excessive, and in the sale of small properties operated as a complete barrier to the transfer of land. The changes which he suggested would also diminish the delay and costs of Chancery proceedings, as the names and residences of all parties entitled to notice respecting the sale of any portion of land would be at once disclosed. This would lay the foundation for applying the doctrine of *market overt* to land, and obviate the lengthened investigation of title required in every case of sale; as with a perfect registry based on the Ordnance Survey, it might safely be enacted that after due notice to all parties on the register a public sale by a party having a power of sale should confer a parliamentary title, the purchase-money, if anybody required it, being lodged in the Court of Chancery. He concluded by quoting the opinion of the Real Property Commissioners, that "the subject of registry of deeds and instruments relating to land exceeded in magnitude and importance all the subjects which they had to consider," and "that the regulations of the Act of General Registry in Ireland were imperfect, and occasioned unnecessary trouble and expense."

The Usury Laws.—Statistics of Pawnbroking.
By Prof. W. N. HANCOCK, LL.D., M.R.I.A.

In the course of some investigations into the condition of the poorer classes in Ireland, Professor Hancock's attention was directed to the state of the trade of lending money among them. He found that while the larger farmers resorted to regular banks to make deposits and obtain loans, there were no banks established for the

smaller farmers and labourers, who were thereby forced to carry their deposits to charitable banks, and obtain their loans from charitable loan-funds at $9\frac{1}{2}$ per cent., or else resort to local usurers at from 25 to 100 per cent. He ascertained that this arose from the laws relating to usury, which made it illegal to charge more than 6 per cent. for loans under 10*l.* where no pawn was made, although pawnbrokers were allowed to charge on some sums more than ten times that per-centage. Some persons alleged the rates of interest which pawnbrokers were allowed to charge to be exorbitant; if this was true, the best remedy would be to leave the trade in money perfectly free, and then the competition of money-lenders would reduce the rate of discount, whether on deposit or on personal security, to the least possible amount. But there were two circumstances which indicated that the rate of interest was not so excessive as it appeared to be; first, the effects produced by the lower scale allowed to be charged in England and Scotland; secondly, the failure of the *Monts de Piété* established in Ireland for the purpose of lending upon pawns on more favourable terms than pawnbrokers. The restricted per-centage allowed to be charged on small sums by the regular pawnbrokers in England and Scotland had set up a class of tradesmen, known in London as “dolly-shop keepers,” and in Scotland as “wee-pawns,” who evaded the law by nominally purchasing from a borrower articles of less value than the licensed pawnbroker will receive, with a tacit agreement that if the latter comes back in a month or six weeks at furthest, he will get his goods on paying the sum lent, and a bonus, this bonus being a penny per week for one shilling, or at the rate of $433\frac{1}{3}$ per cent. per annum. The obvious remedy for the evils of “wee-pawns,” and the other evils connected with the trade of pawnbroking, was, in Professor Hancock’s opinion, to leave that trade perfectly free; let borrower and lender make their own bargain, and let the law not interfere except to enforce *bonâ fide* contracts, and to protect against frauds. As to *Monts de Piété* (whose introduction had been advocated by Mr. Henry John Porter at the Meeting of the British Association in 1840), the attempt to establish them in Ireland had, after the trial of several years, ended in a complete failure. The whole investigation of the facts with regard to pawnbroking, “dolly-shops,” “wee-pawns,” and *Monts de Piété*, taught a lesson of the folly of legislating on different principles for the poor and rich. The real remedy for the evils of the system was to establish the same freedom in lending small sums that had for some years existed with regard to large sums. The defenders of the present state of the usury laws could be reduced to a complete dilemma. For how stood the case? The merchants applied to Parliament for a suspension of the usury laws, on the ground that these laws, instead of keeping down the rate of interest when any commercial crisis tended to raise it above the legal rate, really raised it much higher than it would have risen, compelling them to pay 20 or 30 per cent. when they need only have paid 8 or 10 per cent. If this reasoning were correct, as all economists admitted it to be, could anything be more cruel than to expose the poor to the evils from which rich merchants had been relieved? But if the economists were mistaken, why was the suspension of the usury laws not repealed, and why were pawnbrokers allowed to violate the spirit of these laws? In the commercial crisis of 1847, whilst the Prime Minister advised the Bank Directors not to charge less than 8 per cent. on loans on approved security, to the rich merchants of London, the law made it illegal for any one to lend small sums to poor farmers to help them through the same crisis at a higher rate than 6 per cent. How were they to get money at 6 per cent. when the market rate of interest was 8 per cent.? When merchants were allowed to borrow at 8 per cent., why should farmers and the poor be prohibited from borrowing at the same rate?

On the Discovery of Gold in California.

By Prof. W. N. HANCOCK, LL.D., M.R.I.A.

Professor Hancock proposed to investigate the following questions:—First, on what causes did it depend whether prices in the British dominions would be affected by this discovery? secondly, How could we ascertain whether as a matter-of-fact our prices were affected by it? and thirdly, If our prices were likely to be altered by it, how could we guard against any extensive change in prices being produced? These questions were of immediate and practical importance, for the discovery of the abundant gold and silver mines in America in the sixteenth and seventeenth

centuries produced the most remarkable changes in prices at that period, so that the prices of all commodities were quadrupled in the short space of seventy years. Although this change did not begin to take place till twenty years after the discovery of Potosi, yet a similar change at the present day, if the causes were in existence to produce it, must be expected to happen with much greater rapidity, as the facility of transit and the promptness with which labour and capital were applied to industrial undertakings would bring the produce of the American mines into the European market with much greater rapidity than in past centuries. It must also be recollected that there was not the slightest provision in the present or past arrangements of the British currency to prevent changes in prices being produced to any extent by the gold mines of California if their fertility were sufficient to effect such changes. In investigating the cause of changes in prices, there were two classes of changes to be considered which were perfectly distinct from one another.

Sometimes the prices of particular commodities varied without any corresponding variation in the prices of other commodities. At other times the prices of all commodities partook of simultaneous changes of the same proportion and in the same direction. Changes of the first class arose from causes affecting the value of the particular commodities in the prices of which they occurred. Changes of the second class were quite independent of the value of the commodities, and arose solely from changes in the value of the metal or other commodity that was used as money.

The *price* of a commodity in any place meant its value estimated in the money of that place, or, in other words, the quantity of money that could ordinarily be there received in exchange for it; and this quantity might increase either from the commodity becoming more valuable or from the money becoming less valuable. As gold was the standard of value in England, it followed at once that whatever cause affected the value of gold as a commodity would affect prices in Great Britain; so that it was only necessary to consider whether the discovery of gold in California would affect the value of gold as a commodity. But this depended entirely on the cost of production of gold there. The answer to the first question might be stated in a few words. The extent to which British prices could be affected by the discovery of gold in California depended on the difference between the cost of obtaining gold there and the cost at the least fertile mine now worked, or which continued to be worked after the discovery.

As to the second question, it was manifest that it could not be solved directly. No statistical investigation, however carefully pursued, could enable us to ascertain the cost of production in California; for there the prices of labour, of the use of capital and of raw materials of every kind, were in a state of most rapid fluctuation. It would also be extremely difficult to discover with that certainty which the importance of the question would require, the least fertile mine. But there was fortunately an indirect method of discovering the effect of the Californian gold without sending statisticians to that perilous region; and this indirect method gave results far more certain than any that they could discover for us. Let the price of silver be observed in England where gold was the standard of value, and the price of gold on the Continent where silver was the standard. If it were found that the price of silver was rising in England and the price of gold falling on the Continent by the same amount, it might reasonably be inferred that the Californian discovery was affecting the value of gold. But this conclusion could be corrected and verified by a very simple method. Let there be a systematic set of observations of the prices of all the chief commodities in some place in England—say in London. Select from this list of observations the twelve commodities that were ordinarily most constant in value. Observe whether there was now any simultaneous change going on in the prices of these commodities. If they were found to be all increasing in value by the same amount at the same time, it might be inferred that gold was changing in value; for it was highly improbable that twelve commodities ordinarily constant in value should all change in value to the same extent from causes peculiar to themselves.

Should the result of these observations prove that prices had begun to be affected by the discovery, then it would be necessary to consider the third question—How can we guard against any extreme change in prices being produced? From the manner in which the subject was alluded to in conversation and noticed in the public prints, it would seem that the community in general was ignorant of the

frightful evils which arose when the standard of value, either from natural causes or from the culpable neglect of government, became variable to any serious extent. But those evils were plainly demonstrated by the results of a variable standard in the reigns of Henry the Eighth, James the First and James the Second. The remedy for these evils could be discovered in a very simple way from considering the reason why gold had been maintained as our standard of value. It had been so maintained because for two centuries it had been of all commodities the least variable in value, and therefore the best fitted to serve as the measure of the value of other commodities. Should it now from any cause become variable in value, the same reason that has impelled us hitherto to select it would lead us to take in its place as a standard the commodity which would then become least variable in value. This commodity would, he believed, be found to be silver. Silver was our standard of value for many centuries after the Conquest. It formed a mixed standard along with gold from 1717 till 1774. It was now used as a standard in France, Hamburg and many other European states, and also in the United States of America. There was no reason, therefore, why we should not, if necessary, adopt silver as our standard, and so entirely obviate any variation in prices being produced by the discovery of gold.

On the Sanitary Condition of Darwen, Lancashire, with Suggestions for its Improvement; in a Letter from J. PATON, C.E.

On the Tenure of Land in the Island of Madeira.
By the Very Rev. G. PEACOCK, D.D., Dean of Ely, F.R.S.

The surface of the island of Madeira is singularly corrugated and mountainous; with the exception of a small portion near the level of the sea on the western coast and the table-lands of the Paul de Serra, a very lofty mountain range, there is absolutely no level ground. From the central region of the Curral, which reaches an elevation of more than 6000 feet, a series of steep and precipitous ridges, with deep ravines, the channels of the mountain torrents, radiate in all directions to the sea, reaching it at various elevations, exceeding 2000 feet at Cape Giram about two miles to the west of Funchal, and forming almost everywhere a coast line of great boldness and magnificence. The rocks are entirely volcanic, presenting every variety of basalt, compact, vesicular and scoriaceous—tufas, which are sometimes loose and friable and at others more or less solid and decomposing slowly under the influence of the atmosphere—extensive beds of white lapilli and pumice, intermixed with earthy particles, and not disposed in the order of gravity—beds of volcanic mud in various states of consolidation as affected by the action of the overlying lavas—others of volcanic cinders and fragments of volcanic rocks and also of scorïæ, occasionally of great thickness, which it would be sometimes difficult to distinguish from the products of an iron furnace. The surface which is capable of cultivation, in an island thus physically constituted, bears a small proportion to the whole—more than half of it—the region of the vaccinium and arborescent heaths—is either too barren or too elevated for the successful growth of the cereal, and affords a very scanty pasturage for cattle, sheep and goats. Much of the remainder is either sterile rock or too precipitous for tillage.

Of the parts of the island which are capable of cultivation, a great portion is only maintained in that state by walls and terraces, succeeding each other frequently within the distance of a few feet, which not only divide the several occupations from each other, but serve to protect the vegetable soil from being washed into the ravines and torrents by the violent rains which are known to prevail, in their proper seasons, in a semi-tropical island. The richer soils are found generally in the lower lands near the sea-coast and at the bottom of the ravines.

The finest wines are produced between Funchal and Cama dos Lobos and the Etreito, and in a few other favoured localities on the southern parts of the island. No wines of a superior quality are produced at an elevation exceeding 1000 feet; its cultivation ceases altogether when we reach 2000 feet.

Among other productions of these more favoured regions we find coffee of a very superior quality. Sugar, which was once extensively cultivated, and formed a large

article of export, is grown in sufficient quantity to supply, in the form of molasses, the wants of the labouring population. The arrow-root is of first-rate excellence. The usual fruits of countries bordering on the tropics—the banana, custard-apple, guava, oranges, &c.—are found in great abundance, but rarely cultivated with much care or skill. Wheat, beans and barley, form, besides the vine, the principal articles of cultivation, but the produce is quite insufficient for the wants of the population. Maize is beginning to be grown in small quantities in some parts of the island. A species of yam, which is very productive, is cultivated on lands capable of irrigation on the banks of the torrents. The potato, which formed the principal food of the people and which was produced abundantly in the mountains as well as lower lands, has been attacked with the prevalent disease, and its loss has produced the usual disastrous effects which have been observed in other countries amongst those who depended upon it for their support. Sweet potatoes, cabbage and other garden vegetables, are produced in abundance, but nowhere of first-rate excellence—no attempt, in fact, is made to procure improved seeds or new species of plants or trees. The fruit-trees are such as are naturally produced, and are never grafted. The cultivators follow rigorously the practices of their forefathers, and resist with characteristic obstinacy all attempts at innovation. They are too poor or depressed, or too imperfectly educated, to look out either for new methods of cultivation or new articles of produce.

The peculiar tenure of land in Madeira (which prevails more or less in Portugal, Spain and Italy, a relic of the dominion of the Romans, and which once prevailed, though under a modified form, in France) is intimately connected with the condition both of the cultivation and of the people. During my stay in Madeira, in the course of the last winter, I paid particular attention to the conditions of this tenure, and to the consequences which it appeared to produce. It is a subject about which it is difficult to procure very accurate or precise information. There are no Portuguese books (or at all events none which I could procure) which describe it. There are no published statistical details, and none which are easily procurable. The codes also of the Portuguese law, though excellent in principle, and such as, if executed, would be very effective in their operation, are very imperfectly or very corruptly administered, so as to place in many important cases the theory and the practice in striking contrast with each other. Under such circumstances I felt myself compelled to depend partly upon personal inquiries and observations, and partly upon a series of replies made by a most intelligent Portuguese gentleman to queries which I had prepared very carefully so as to include nearly all the points which could affect the relation of landlord and tenant, and the effects which they produced upon the cultivation of the land and the condition of the people.

By the ancient laws of Portugal, a proprietor could alienate by will to a stranger, away from his natural and compulsory heirs (children or grandchildren, father or mother, grandfather or grandmother), one-third part of his possessions; and he who had no such heirs could dispose of them at his pleasure. A custom afterwards arose, under the authority of the Church, of instituting *vinculos*, or perpetual entails, upon the natural heirs, on condition of providing for the performance of certain masses and distributing certain alms for ever for the souls of the entailers and his progenitors. All that remained when these claims were satisfied became the property of the possessor for life, and passed in succession to his heirs, male or female, one or both, according to the conditions of the *vinculo*, and upon their failure reverted to the Crown. During the continuance of the entail the estate could not be charged in any way whatever, nor let for any period extending beyond four years of the life in possession, or beyond eighteen years of the same event, with the especial consent of the heir in succession, who claimed, in both cases, the rent with the inheritance: no provision could be made for other members of the family: it continued for ever a life possession, and a life possession only in the strictest sense of the term.

The union of several *vinculos* constituted a *morgado*, and the same term is applied in the Portuguese language both to the possession and possessor.

The effect of this system, whether due to the influence of the Church or to the passion, so natural to mankind, to transmit their name and influence, in connexion with their possessions, to their most distant posterity, was the absorption of nearly the whole territory (which was not in the possession of the Crown, or of religious establishments) in the hands of the *morgados*. Their further institution, however, was forbidden by a

law of Don José the First in 1770, under the bold but generally wise administration of the Marquis de Pombal, which declared the system to be contrary to the just rights of property, and to the just claims of the other members of the family. A further and a more serious assault upon the system was made by the law of Don Pedro of the 4th of April 1832, which removed the entail from every separate *vinculo* which produced a smaller revenue than 200 dollars a year, and from every *morgado* or union of *vinculos* of less than twice the amount. The effect of this law is already beginning to be felt in sales of land to English and other capitalists, and the range of its operation is rapidly extending in consequence of the great depreciation of land in Madeira, from the rapid reduction of the value of the wine, which is the staple product of the island. It would appear, however, from the most careful inquiries which I could make (though correct information on the subject is not easily obtainable), that nearly four-fifths of the cultivated lands are still under the operation of the *vinculo*.

There is some land (but not of great extent) belonging to three convents of nuns, the only religious communities which survived the revolution of 1821: some is in possession of the Crown; there are also some customary freeholds held by peasants in the mountains; but the greatest portion of the mountain pasture is in possession of the *camaras* or municipal bodies of the different parishes, and is commonable by all the occupiers of lands within their limits. So defective, however, is the execution of the law in every part of the island, that those districts are treated as common property, whether for pasturing cattle or collecting fuel, by cutting furze, broom, brushwood or timber, without any system or control. From this cause the noble forests of the interior, consisting of the til, the vignatico, the folhada (all species of laurels), and the tree heaths, many of them of gigantic size, are rapidly disappearing. They served an important purpose in collecting the moisture and feeding the springs and torrents in those lofty regions, which are a principal source of the fertility of the cultivated regions below, to which they are conducted by an elaborate system of *levadas* or water-courses, which have been in the course of construction during many ages, and many of which are still in progress.

The *morgados* formerly possessed country-houses, sometimes of great extent and magnificence, always with a chapel attached to them, where the masses required by the deed of their foundation were performed. They expected and received many acts of homage and of service from their tenantry, who regarded them as their masters or feudal lords. They brought offerings of their produce upon his marriage or the birth of his heir. They brought fowls to him at Christmas, and a portion of the head of every pig which they killed: if he removed from his country to his town house, or in his other journeyings, they bore his hammock (for there are no carriages in Madeira) and his baggage; during the war also, when the island was in English occupation, and the resort of our Eastern convoys and ships of war, and when their wines bore an exorbitant price, they were generally rich and prosperous, and profuse in their expenditure. The revolution of 1821 swept away these and other feudal distinctions, and loosened the bonds which connected the rich and the poor, whilst the rapid fall of the price of their wines reduced their incomes far below the scale of expenditure which most of them had adopted. They began in consequence to anticipate their revenues, by selling the reversion of their crops for years to come to the English merchants. The consequences were equally ruinous to themselves and to the progress of improvement; they became more and more severe in their exactions from their tenantry; their residence in their country-houses became neither convenient nor safe, and they have since been very generally abandoned.

The *caseiros* or occupiers, the successors of the Roman *coloni*, held their lands universally upon the *metayer* system, where the gross produce is divided equally between them and the landlord. They build their own cottages, the walls (of rough masses of basalt or tufa) which surround their occupations or support their soil: they plant their own vines, chestnut, orange, or other fruit-trees; if water be brought to their lands from the *levadas* it is at their own expense; whatever, in fact, is necessary to bring their land into cultivation is done by them. These improvements, provided they are useful, or even when they are not so (for though there is a distinction between them in law there is none in practice), or *bemfeitorias*, are the absolute property of the *caseiro*, who cannot be removed from his occupation until his land-

lord has paid him their full value as ascertained by two public valuers appointed by the *camara* or municipality of the district. The value thus assessed far exceeds in most cases the real or marketable value (such as the *caseiro* would obtain if he should sell them, as he is authorized to do, to another person), and it is only in very extraordinary cases that the landlord becomes the purchaser. The effect of this regulation is to give the *caseiro* very nearly a perfect fixity of tenure.

The other relations of the landlord and tenant are regulated entirely by the law, or rather the custom of the country, and rarely, if ever, by special contract; a lease is absolutely unknown, at least as far as concerns the occupation of land; the lord takes one-half of the wine when it issues from the wine-press, one-half of the corn when trodden out from the straw on the threshing-floor, as well as one-half of the straw itself, one-half of the sugar-cane, fruits, garden and other produce, one-half of the grass or of the very various produce which is sold as grass, which is not consumed on the premises. It should be remarked, however, that before this division is made, the tithe of all the produce is claimed by the officers of the Government. The more liberal landlords give the seed, and do not claim the smaller articles of produce; but those who are very needy or have leased their claims to a *rendeiro* or middle-man, as is very commonly the case, exact their rights with great and oppressive severity.

The produce of cattle, pigs, and poultry which are fed on the farm belong entirely to the *caseiro*. Sometimes the landlords furnish the cattle, &c. at a price agreed upon, and divide the profit upon them when sold, equally, or in some proportion agreed upon with the tenant; it is not unusual, also, to agree upon a money price of the corn and other produce, before it is gathered, leaving the risks to the tenant: this is very rarely done in the case of wine. The consequences of these arrangements, where a kind and confidential feeling does not exist (as is very rarely the case) between the landlord and the tenant, are such as might be anticipated: frauds become rather the rule than the exception. The ears of corn and other articles are abstracted and concealed or sold. If the factor who watches the interests of the landlord or the middle-man is too vigilant he is threatened, and in some cases murdered. If a landlord resides upon his estate in the country and exercises too close an inspection of the productions of his tenants, he is subjected to annoyances and losses, which render his residence amongst them uncomfortable at least, if not dangerous. The occupations of the tenants are generally extremely small. In the richer and more productive districts, they rarely reach an acre of ground; much more frequently not one-half or even one-tenth of that quantity. There is, in fact, hardly any limit to the extent of these sub-divisions. If a *caseiro* dies, his children succeed to the inheritance in common, and either divide it, building cottages on their occupations, or hold it in common; for it rarely happens that they possess sufficient money to be able to buy up the portions of the *benfiteiros* which belong to the other claimants.

The cultivation is generally of the rudest kind. In large districts we find that wheat has been grown on the same land for twenty or thirty years in succession. The crops, as may be expected, are extremely poor. I could obtain no answer to my inquiries respecting the average produce per acre: I should think that it rarely reached fourteen bushels—in most cases not half the number. Many of the crops which I examined in April last, a very favourable season, would hardly, in this country, be considered worth the trouble of reaping.

On the mountains, at elevations not exceeding 2000 or 2500 feet, the broom and furze is burnt once in six or seven years, and the ashes manure the land for a crop of rye of the most miserable description. The land is said to be exhausted by the effort, and the experiment is not repeated until the end of another septennial period. In the smaller occupations, we find the mixture of productions which we should naturally look for in gardens, without the clean and laborious cultivation which is essential to its success: wheat or barley, sugar-cane, vines, arrow-root, coffee in many protected situations, vegetables, peach, fig and orange trees, are intermixed without order or arrangement; weeds of all kinds are allowed to grow freely, particularly under the vines, and are cut from time to time, and used or sold as fodder for the cattle; the succession also of many of their productions in a country with a temperature of perpetual spring or summer is almost independent of the season; but no advantage seems to be taken of this singular felicity of the climate: no selection of seeds or plants, no proper pruning, no grafting, no system, no horticultural or

agricultural knowledge; they follow the practices which they have been taught by their fathers, and resist or neglect all change or instruction with a pertinacity which no prospect of profit can overcome. It is very common to see occupations uncultivated, but never abandoned; for though the law gives the landlord a control over the cultivation, and the means of punishing or ejecting a refractory or a negligent tenant, it can only be practically enforced through the purchase of the *bemfeitorias*. The *caseiro* may be engaged in other occupations more profitable than the cultivation of his land, or influenced by other motives; whilst the landlord will acquiesce in the loss of the produce which the land is capable of yielding rather than incur the cost of the purchase of improvements, or what are so considered, more particularly when estimated far beyond their value. There are considerable tracts in some of the most favoured situations of the island which are in the situation I have described.

Of all the productions of the island the vine is the most extensively and most carefully cultivated; the soil most adapted to it is a mixture of the red and yellow tufa, called the *saibro* and the *pedra molle*, the latter of which is very light and loose, and would be easily washed away if not mixed with other soil; another soil, called the *cascalha*, a decomposing basaltic conglomerate, is also well adapted to the purpose. The clayey soil, called *massapez*, unless largely mixed with lighter and more friable materials, must be carefully avoided. The vines are planted in trenches dug to the depth of five or six feet; they are trained upon a framework formed by the stalks of the reed (*Arundo sagittata*) supported upon wooden posts or stone piers at an elevation of five or six feet from the ground, and tied together with twigs of the red willow (*Salix rubra*): vegetables and weeds of all kinds are cultivated or allowed to grow beneath them, which are generally cleared away in the summer season when the vines are in bearing. It is a necessary effect of this system that the vines are pruned not to but from the root, so as to add during every year to the length of the ancient stem through which the sap is transmitted to the fruit: they thus become weaker instead of stronger with the increase of age; and at the end of little more than twenty years a vineyard must be destroyed and replanted. The fruit also decreases in flavour and richness the further it is removed from the ground,—a fact which the French and German vine-growers fully understand, and consequently have adopted a totally different system. The vineyards are seldom of great extent, the largest not exceeding three or four acres; they are very rarely if ever (except in the gardens of *quintas* in and around Funchal) cultivated by the proprietors, but almost always by *caseiros*, the same division of the produce prevailing in the richest and the poorest vineyards; their produce will vary from one to three pipes per acre. The price to the producer during the last year varied from about 40 dollars in the best districts, to 10 or 12 dollars per pipe; towards the end of the last war the price was nearly three times as much. The vine in its progress to maturity is exposed to a variety of enemies; the innumerable multitude of lizards, rats and bees destroy generally one-sixth part of the produce. The grapes, which are allowed to hang until they are perfectly matured, produce the wines of the richest flavour, and the wine-merchants will frequently double the price per *baril* for wines which are thus preserved; but when the grapes of the neighbouring vineyards are gathered, their enemies crowd to the plunder of those which remain: it is only by speedily gathering them that they can be saved from entire destruction.

Most of the occupations are too small, or in situations too precipitous for the use of the plough, which is an instrument of the rudest materials and form; but even when it might be advantageously applied it is not generally used: the favourite and almost exclusive instrument of cultivation is the *enxada*, a slightly incurved pick-axe, which they use with great dexterity to break up rather than upturn the surface of the soil; the spade is rarely used, and they have neither rakes nor harrows. The soil is generally light and friable, and rarely requires the careful breaking-up and trituration which is necessary in other countries. The *caseiro* is not allowed to sell the straw or manure which is produced on his tenure; but little care is used either in preserving or preparing it, and the use of artificial manures is altogether unknown. Water in this, as in all warm countries, is the most essential element of fertility. In Madeira, it is diverted into channels or *levadas*, from the mountain-stream at high elevations, and conducted by them to the several occupations in the cultivated districts which have acquired by purchase or hiring the privilege of using it. It is di-

vided into a monthly cycle of *giri* or turns of one hour each, and conducted, in its proper succession, into the several channels which connect it with the separate occupations. Of the public works of Madeira, the *levadas* are much the most considerable and much the best managed.

The oxen, of which the breed in the island is very active and well-formed, are the only beasts of draught. Carts and carriages, and even wheelbarrows, are unknown: heavy burdens are drawn on sledges by two oxen: lighter burdens of all kinds are carried on the heads of men and women. One-third at least of the inhabitants of the island are thus employed as beasts of burden, carrying fuel from the mountains, articles of produce and skins of wine from their farms. There are some portions of road which are very carefully constructed and paved; but there is hardly a single continuous well-formed road to be found; none to connect, for purposes of draught, the distant parts of the island with each other. Their steepness also in many cases is formidable. There is a principal road in the northern part of the island near St. Anne, with an inclination of 27° . One of the most carefully-paved roads, only recently made, leading from Funchal to the mountains, has an inclination of 23° : a road with an inclination of 1 in 4 is considered practicable and convenient: the Simplon has an inclination of 1 in 12, which is nearly the limit of that which is practicable for carriages. All the horses, or rather ponies, are shod with particular reference to these steep and precipitous roads.

The condition of the people in the villages and remote districts is miserable in the extreme, more particularly since the failure of the potatoes. With the exception of Skibbereen and a few other places in Ireland, I have nowhere seen such squalid poverty: their food is chiefly maize, pumpkins, salt fish, and the tunny-fish, which is caught in great abundance off the island. Begging is universal and very importunate, yet the people are generally very patient and courteous. They never meet a stranger without a salutation. They are contented with the means of existence, and would maintain no steady or continuous exertion to attain much beyond it; but the occasional labour which they will go through, more particularly in bearing heavy burdens across the mountains, is astonishing. —

On a Comparative Statement of Prices and Wages during the Years from 1842 to 1849. By G. R. PORTER, F.R.S.

The usefulness, not only to ourselves, but to those who will come after us, of records such as those which I have now to bring forward as a sample, will be apparent to every one who has at any time attempted to investigate the comparative condition at different periods of our working population. To begin with what is emphatically called "the staff of life," and the price of which is a thing of the very first importance to those who depend upon daily or weekly wages. The four-pound loaf of bread sold in the bakers' shops in London has been, in the month of July of each year from 1842 to 1849, as follows:—

1842.....	9½d.	1846.....	8½d.
1843.....	7½d.	1847.....	11½d.
1844.....	8½d.	1848.....	7½d.
1845.....	7½d.	1849.....	7d.

When it is considered that from one-half to three-fourths of the expenditure of the most numerous class of the people is for this one article, it cannot be held of light importance that a saving of 25 per cent. is made in its cost. Such a saving to the family of a working man—consisting of himself, his wife, and four children—can hardly be less than 2s. per week, which is too often a very considerable proportion of the man's earnings; so that it will greatly depend upon this head of expenditure whether or not he and his family are able to provide themselves with decent clothing and with other matters, which, although perhaps not absolutely, nor equally necessary to the support of life, are yet most important towards the comfort and contentment of the family. The price of meat is, unfortunately, not a matter of such universal interest as the cost of bread; and it is to be feared that even in ordinarily prosperous times there are very many of our fellow-subjects who are forced to forego its use. But it must be obvious that the numbers thus subjected to privation will, as already explained, greatly depend upon the cost of bread,—while in large towns it will be found upon inquiry that few or none are, except in the very dearest times, deprived of the occasional or perhaps the habitual use of meat. The prices as quoted

in the accounts of markets cannot be taken as the prices actually paid for their retail purchases by the families of working men; they will, however, afford accurate means for comparison, since no doubt the wholesale price of the carcass must indicate the retail prices charged for its constituents. The following prices are those given for the primest beef (Scots) and South Down mutton at Smithfield in the month of June in each year, 1842 to 1849:—

	Beef.	Mutton.
1842.....	4s. 8d.	4s. 3d.
1843.....	3s. 10½d.	4s. 1½d.
1844.....	3s. 8½d.	3s. 10d.
1845.....	4s. 4d.	4s. 9d.
1846.....	3s. 9d.	4s. 3d.
1847.....	5s. 6d.	5s. 7d.
1848.....	3s. 10d.	4s. 11d.
1849.....	3s. 5d.	4s. 2d.

The prices paid for beef and mutton at St. Thomas's Hospital in the same years, at Lady-day and Michaelmas, have been as follows:—

	Beef.		Mutton.	
	Lady-day.	Michaelmas.	Lady-day.	Michaelmas.
	s. d.	s. d.	s. d.	s. d.
1842	3 4	3 0	3 8	3 4
1843	2 8	3 0	3 0	3 4
1844	2 8	2 8	3 0	3 4
1845	2 8	3 4	3 4	4 0
1846	3 8	3 4	4 4	4 0
1847	3 8	3 10	4 4	4 6
1848	4 0	3 4	4 8	4 0
1849	3 0	...	3 8	...

The retail prices paid by the working classes for other articles of food and for groceries, in a populous district of London, from 1844 to 1848, were as under:—

	1844.	1845.	1846.	1847.	1848.
Tea.....per lb.	5s. to 7s.	5s. to 7s.	5s. to 7s.	4s. to 6s.	4s. to 6s.
Raw Sugar....."	6d. to 7d.	5d. to 6d.	5d. to 6d.	4d. to 5d.	4d. to 5d.
Refined Sugar .."	7½d. to 8½d.	6½d. to 7½d.	6½d. to 7½d.	5½d. to 6½d.	5d. to 6d.
Coffee....."	1s. 8d. to 2s. 6d.	1s. 8d. to 2s. 6d.	1s. 8d. to 2s. 6d.	1s. 4d. to 2s.	1s. 4d. to 2s.
Cocoa....."	6d. to 8d.	6d. to 7d.	6d. to 7d.	6d. to 7d.	6d. to 7d.
Rice....."	2d. to 3d.	2d. to 4d.	3d. to 4d.	3d. to 4d.	2d. to 3d.
Currants....."	5d. to 7d.	5d. to 7d.	5d. to 7d.	5d. to 7d.	4d. to 6d.
Raisins....."	5d. to 7d.	5d. to 7d.	5d. to 7d.	4d. to 6d.	4d. to 6d.
Butter....."	10d.	10d.	10d.	9d.
Cheshire Cheese .."	9d.	9d.	9d.	9d.
Derby Cheese .."	8d.	8d.	8d.	8d.
Dutch Cheese .."	6d.	6d.	6d.	6d.
Lard....."	9d.	9d.	10d.	10d.
Bacon.....per cwt.	62s. to 68s.	63s. to 70s.	82s. to 88s.	75s. to 81s.
Eggs.....per 120	7s. 6d.	7s. 6d.	7s. 6d.	7s. 6d.

The retail prices of such articles of the qualities usually consumed by the working classes at Birmingham during the years from 1844 to June 1849, have been procured for me by an inhabitant of this town:—

	1844.	1845.	1846.	1847.	1848.	June 1849.
	s. d.	s. d.	s. d.	s. d.	s. d.	s. d.
Tea.....per lb.	5 0	5 0	5 0	5 0	4 6	4 0
Sugar, raw....."	0 7	0 6½	0 5½	0 5½	0 5	0 4½
Sugar, refined .."	0 9	0 9	0 8	0 7½	0 6½	0 6
Coffee....."	1 8	1 8	1 8	1 8	1 6	1 4
Cocoa....."	0 10	0 8	0 8	0 8	0 8	0 7
Rice, Patna"	0 3	0 3	0 3	0 4	0 3	0 2½
Oatmeal....."	0 3	0 3	0 3	0 3½	0 3	0 3
Currants....."	0 6	0 6	0 6	0 7	0 6	0 6
Raisins....."	0 6	0 6	0 6	0 6	0 5	0 5
Butter....."	0 10	0 11	1 0	1 0	1 0	0 10
Eggs, number of, for 1s.....	20	20	20	20	20	24

In the course of inquiries made among London tradesmen while collecting the foregoing lists of prices, it was stated that as respects cheese, the working-classes seldom or never buy it by weight, but apply at the shop for sixpennyworth or three-pennyworth, or whatever may be the sum to be laid out, the dearness or cheapness of the article affecting the quantity that they receive for their money. Another fact of the same nature was ascertained from the proprietor of a very large establishment for the sale of linens, woollens, haberdashery, and the like goods. The working man or woman is accustomed to pay certain prices for certain articles, and does not vary the outlay with the varying markets, but buys a 3s. or 5s. hat or bonnet, a shirt or shift for so much, and so on through the whole list of articles of clothing. The benefit of cheapness reaches them in the quality of their purchases; and as the tendency has for very many years been towards lessened prices, we now see—such at least is the case in London—that the working classes are better clad than formerly, keeping in this respect their relative position with the more easy classes, whose dress—especially among ladies—is generally not only better in quality but actually more costly than when the articles used were of much higher prices than now. When engaged upon an inquiry similar to the present—fifteen years ago—I was informed by a person who gave constant employment to 1200 people, men and women, in making up articles of clothing used by the working classes, that, taking one article with another, the materials used then cost not more than one-half what they had cost at the close of the war in 1815; and we know that, since 1834, there has been a further and very great abatement in the cost of most if not all such materials. Strong cotton cloths, the wholesale price of which in 1810 was 10*d.* per yard, sold in 1820 for 9*d.*, had fallen in 1833 to 4*d.*, and may now be bought at from 2*d.* to 2½*d.* per yard. Printed calico, which sold in 1810 at 2*s.* 2*d.*, in 1820 at 1*s.* 4*d.*, in 1830, the excise duty having been removed, at 6*d.* to 8*d.*, may now be bought at from 3*s.* 6*d.* to 6*s.* per piece of 28½ yards, or from 1½*d.* to 3½*d.* per yard. The increased use of cotton in this country, so far beyond the increase in our export of cotton goods, proves that the people, and especially the working people, who are the great consumers of cotton goods among us, have fully profited by their progressive cheapening. The consumption of such articles as are of home production we have no satisfactory means of determining; but we may feel quite certain that as respects such of them as are articles of necessity, as well as those which have become so through the usages of society, a fall in price when unaccompanied by circumstances that oppress the people, must be accompanied by an increase in their use. If we had any doubts upon this head, they must, however, be dispelled when we find that other articles of which, being brought from distant countries, we know the quantities used, are so used in greatly increased quantities. The consumption in each year from 1842 to 1848, in those articles of which retail prices have been given and which are imported, have been,—

	1842.	1843.	1844.	1845.	1846.	1847.	1848.
Sugar.....cwt.	3,668,466	4,028,307	4,129,443	4,856,604	5,220,248	5,779,508	6,208,872
Tea.....lb.	37,355,911	40,293,393	41,363,770	44,193,433	46,740,344	46,314,521	48,733,971
Coffee....."	23,519,646	29,979,404	31,352,382	34,293,190	36,754,578	37,441,372	37,106,292
Cocoa.....cwt.	2,246,569	2,547,934	2,589,977	2,579,497	2,951,206	3,079,198	2,935,479
Rice.....cwt.	396,922	316,359	432,480	372,274	545,883	971,694	925,731
Currants .."	195,379	254,330	284,694	309,485	353,761	331,236	350,500
Raisins"	186,240	236,826	202,230	204,960	238,255	212,024	228,542

It appears thus, that a reduction in the retail price of sugar from 7*d.* to 4½*d.* for raw and from 9*d.* to 6*d.* for refined sugar, has increased the consumption since 1844 by 2,079,429 cwt., or 50 per cent. The reduction of 1*s.* per pound on tea, viz. from 5*s.* to 4*s.*, has caused an additional consumption of 7,372,201 pounds, or 18 per cent. The retail price of coffee has fallen from 1*s.* 8*d.* to 1*s.* 4*d.*, and the consumption has been augmented by 5,753,910 pounds, or 18 per cent.; thus adding very materially to the comforts of the working classes, and chiefly the artizan class, among whom the increased quantities here noticed have principally been used. From what has been already stated, we might almost necessarily infer that the people generally are now in a condition of comparative comfort.

On the Agricultural Statistics of Ireland. By G. R. PORTER, F.R.S.

A volume of considerable interest and importance, entitled 'Returns of Agricultural Produce in Ireland in the year 1848,' has recently been distributed to the members of the two Houses of Parliament, and it is thought that a short abstract of its contents may prove interesting to the Section. This volume more than fulfils the promise set forth in its title-page, since it comprises also the returns of agricultural stock and produce in the preceding year, 1847, and thus enables us to draw a comparison between the two years as regards this most important branch of the national industry, under circumstances which give to such a comparison an interest far greater than it would have possessed at almost any other period. The returns have been obtained at the desire of Lord Clarendon, under the direction of Capt. Larcom of the Royal Engineers, who, it will be remembered, read before this Section at the meeting of the Association held at Cork in 1843, a valuable and elaborate paper on the Census of the Population of Ireland in 1841, in which paper a considerable amount of information was given that will admit of the comparison as respects some matters connected with agriculture being carried back to the year 1841. The returns embraced at both periods the number of farms or holdings, distinguished in different classes, according to their acreage contents, information of deep importance considering the faulty, it might rather be said, the fatal subdivision of the soil in that island; and it must be gratifying to learn that a change in this respect is going forward, if not so rapidly as could be wished, yet more rapidly than could have been expected, from the known tenacity wherewith the Irish cottier had previously adhered to his patch of ground. The condition of the country in this respect in each of the three years above-mentioned was—

	1841.	1847.	1848.
Farms from 1 to 5 acres	306,915	125,926	101,779
" 5 " 15 " ...	251,128	253,630	225,251
" 15 " 30 " ...	78,954	150,999	146,725
Above 30 acres	48,312	137,147	140,817
Total number ...	685,309	667,702	614,572

The number of holdings not exceeding an acre were, in 1847, 62,447; in 1848, 44,262.

The paper read by Capt. Larcom on the census returns did not give the number of these small holdings in 1841; but we may fairly presume that it must have been greatly beyond the number ascertained in 1847, seeing that the next smallest description of farms, those of one to five acres, had then decreased in the proportion of three-fifths, while those above thirty acres have increased in a threefold proportion. The next census returns, which also will most probably be made under the direction of Capt. Larcom, will doubtless exhibit the effect which this change produces upon the course of employment. The fact has on previous occasions been noticed, that while in England and Scotland the proportionate number of the population employed in raising food has been decreasing in a very remarkable manner, the contrary result has been experienced in Ireland. It was ascertained, at the census of 1841, that in Great Britain 1000 persons, engaged as occupiers and labourers in raising food, provided for the wants in that respect of themselves and of 2984 other persons, while in Ireland, the like number of persons, viz. 1000 so engaged, provided food for no more than 511 persons beyond themselves. In 1831, the number of occupiers not employing labourers—the lowest description of farmers—in England was 94,883 out of a population of 13,000,000, whereas in Ireland, a population of 7,700,000 furnished 564,274 of such small farmers. A great part of these have changed, or it is to be hoped will change, their condition by becoming hired labourers for others; and as their employers will necessarily be in the possession of some capital, the labour employed by them will be rendered more effective than it could be under the old order of things, where the farmer of a mere patch of ground had usually little or nothing

more than his bodily exertions to assist in developing the resources of the soil. The amount of land under cultivation applied to the production of different kinds of food and the actual produce in each of the years 1847 and 1848 are given in the returns as follows:—

	1847.		1848.	
	Acres.	Produce. Qrs.	Acres.	Produce. Qrs.
Wheat	743,871	2,926,733	565,746	1,555,500
Oats	2,200,870	11,521,606	1,922,406	9,050,490
Barley	283,587	1,379,029	243,235	1,135,120
Bere	49,068	274,016	53,058	263,415
Rye	12,415	63,094	21,502	105,375
Beans and pease	23,768	84,456	50,749	172,508
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Total grain and pulse .	3,313,579	16,248,934	2,856,696	12,282,408
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Potatoes	284,116	Barrels, 20 st. 16,385,562	742,899	Barrels. 23,046,512
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Turnips.....	370,344	Tons. 5,760,616	255,058	Tons. 3,643,074
Mangel-wurzel	13,766	247,269	12,588	220,875
Carrots	59,512	729,064	5,994	75,528
Parsnips.....			2,548	23,091
Cabbage.....			24,114	325,763
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Flax.....	58,312	Cwts. 349,872	53,863	Cwts. 258,608
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Meadow and clover...	1,138,946	Tons. 2,190,317	1,154,302	Tons. 2,287,133
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	5,238,575		5,108,062	

It is necessary to explain, that owing to the unsettled state of the country it was found impossible to collect returns in the counties of Waterford and Tipperary, so that in drawing a comparison between the result of that year and of 1847, we must deduct the returns for those two counties:—

The total number of acres under cultivation in 1847 was found to be... 5,238,575
If we deduct therefrom the area cultivated in Waterford and Tipperary 432,977

The remainder will show the extent which is fairly to be brought into the comparison, viz. 4,805,598
The average under cultivation in 1848 was 5,108,062

Showing the gratifying fact that an increase has been made in one year of 302,464 acres, exclusive of the two counties here mentioned. If the increase in those counties has kept pace with that of the remainder of Ireland, the increased breadth of land brought under cultivation in one year has amounted to 329,715 acres, or more than 6 per cent.

The produce in 1847, if we deduct that of Waterford and Tipperary, was—

Wheat	2,389,815 qrs.	Potatoes.....	15,102,828 brls. 20st.
Oats.....	10,950,414 „	Turnips.....	5,146,490 tons.
Barley	1,251,432 „	Green crops.....	913,439 „
Bere	230,144 „	Flax.....	347,856 cwt.
Rye	58,632 „	Hay.....	2,001,673 tons.
Beans	82,402 „		
	14,962,839 „		

The proportions in which the land was employed for different purposes in 1848 on farms of different extent, are given as follows:—

	Under 1 acre.	1 to 5 acres.	5 to 15 acres.	15 to 30 acres.	Above 30 acres.
Wheat	6.98	9.90	9.26	10.10	12.29
Oats	26.54	41.07	45.46	42.71	32.34
Barley, bere and rye .	13.46	9.27	7.18	6.35	5.55
Beans and pease	0.94	1.01	0.98	1.04	0.97
Potatoes	37.60	23.24	17.95	15.84	11.95
Turnips	4.44	4.10	4.20	4.49	5.57
Other green crops	5.71	0.98	0.81	0.78	0.93
Flax	0.25	0.60	1.18	1.39	0.89
Meadow and clover .	4.08	9.83	12.98	17.30	29.51
	100.00	100.00	100.00	100.00	100.00

The stock of various kinds that existed at the time of the last census (1841) and in 1847 and 1848—

	1841.	1847.	1848.
Horses and Mules	552,569	498,221	499,343
Asses	90,315	112,029	105,017
Horned Cattle	1,840,025	2,367,139	2,481,501
Sheep	2,091,199	1,981,635	1,809,107
Pigs	1,353,101	517,476	549,583
Poultry	8,334,427	4,956,148	5,889,412

afford a strong commentary on the distress occasioned by the failure of the potato harvest. It appears, that comparing 1847 with 1841, the number of horses was less by 54,348, but the deficiency in farms not exceeding fifteen acres, amounted to 163,622, while there was actually an increase on farms above that area of 109,344. Of asses there was an increase of 21,714; but on the small farms there was a falling off in the number of these animals amounting to 32,955, while there was an increase on the larger holdings of 54,669. With respect to horned cattle, there was an increase of 527,114, but this was wholly experienced on the larger farms, there having been on those not exceeding fifteen acres, fewer in 1847 than in 1841 by 336,471, and consequently more on the larger holdings by 863,585. The number of sheep was less on the whole in 1847 than in 1841 by 109,565, but the deficiency on the small farms was 529,226, while there was an increase on the larger. The greatest deficiency has been experienced in regard to pigs and poultry, which in Ireland are especially domestic animals, and, as might be expected, the falling off is found chiefly among the cottier class. In the larger farms—those above thirty acres in extent,—there were 42,643 more pigs in 1847 than in 1841, whereas in all the smaller holdings the difference was very greatly in the other direction. On farms not exceeding one acre the numbers were 295,048 in 1841, and only 19,108 in 1847. On farms from one to five acres, there were 251,587 in 1841, and only 21,422 in 1847. In the next division, between five and fifteen acres, the numbers were 350,825 in 1841, and no more than 80,098 in 1847. Persons holding from fifteen to thirty acres kept in 1841, 215,340, and only 113,864 in 1847; whilst on farms above that size, the numbers, which were 240,301 in 1841, had advanced to 282,984 in 1847. The entire deficiency of this description of stock between the two periods was 835,625, or more than 60 per cent. The diminished number of poultry was 3,378,279 upon 8,334,427, or 40 per cent., which, as in the case of the pigs, applied entirely to the smaller farms. On those above fifteen acres there was an increased number, amounting to 1,048,974, showing that the lessened number on the smaller farms was 4,427,253. The lessened number of pigs is clearly referable to the failure of the food upon which those animals are usually kept in the cabins of the peasantry; and as regards poultry, it could

hardly be expected that a starving people should continue to rear things so easily convertible into food, or into that which would procure food for the owners. These facts, which are proved beyond controversy by the inquiries of the Irish government, place in a very conspicuous light the disadvantage of peasant holdings, as compared with farms which from their extent require to be cultivated by persons who, possessing some capital, are not driven, on the occurrence of the first calamitous season, to measures destructive of their own future prosperity and injurious to the public at large. The question of the advantage, or otherwise, of maintaining a class of peasant proprietors, is one upon which it would not be advisable to dilate on this occasion, but the figures brought forward in the returns under examination appear to be so important, as exhibiting the consequences of farming without the needful appliances, that it was impossible to pass them by without this one word of comment. The table exhibiting the number of acres devoted in 1847 and 1848 respectively to the production of the different cereal grains, shows a result for which we could hardly have been prepared. There was a falling off in the breadth of wheat sown of 178,125 acres, or 24 per cent. upon the quantity in 1847. Of oats there was a lessened sowing of 278,464 acres, or $12\frac{1}{2}$ per cent. Of barley the cultivation was lessened by 40,352 acres, or nearly 14 per cent. On the other hand, the tendency to continue dependent for a great part of their daily food upon potatoes, has been shown by the Irish peasantry in the marked increase of the land devoted to their growth, which amounted to 458,783 acres, or 160 per cent. upon the number of acres so employed in 1847! We hear but little of injury sustained by this root at present, and may expect that the misery through which that peasantry had to pass, consequent upon the destruction of their staple produce, will be forgotten, and that they may be willing to remain in dependence upon the success of this lowest description of food, and thus be liable at any time to a recurrence of the horrors of famine. We are now for the first time in the history of this country enabled to record, with anything approaching to accuracy, the actual and comparative result of two consecutive harvests. The result is such as to prove—if indeed any proof to that effect could be required—of how much, of how vital importance it is to know the truth upon this most momentous subject. We have seen that the breadth of land devoted in 1848 to the cultivation of the cereal grains was much less than in the previous year, and the figures which record the result of that cultivation, serve to show that the actual produce of the land in all its most important objects, was such as greatly to aggravate the evil thence to be expected. It appears upon calculation that the produce of the cereal grains in bushels, and of potatoes in tons, in each of the two years was as follows:—

	1847.	1848.
Wheat.....bushels	31·4	22·0
Barley	39·0	37·3
Oats..... "	41·8	37·6
Bere..... "	44·6	39·7
Rye	40·6	39·2
Potatoes ... tons...	7·28	3·87

If the deficiency here shown were equally great in Great Britain, we can be at no loss to account for the very large importations of foreign grain imported during the twelve months from August 1848 to August 1849, and which importations, great as they have been, would seem to be in no degree beyond our requirements. The quantities so entered have been as follows:—

Wheat.....	4,323,645	quarters.
Barley.....	1,323,827	"
Oats	1,221,883	"
Rye	220,829	"
Pease.....	266,475	"
Beans	531,177	"
Maize	2,287,283	"
Wheat Flour, 3,508,375 cwt.=	1,002,393	"

Total..... 11,177,512 quarters.

Contributions to the Statistics of Sugar produced in India.

By Lieut.-Colonel SYKES, F.R.S.

Colonel Sykes stated that the Government of India having called for returns of the quantity of sugar produced in the different collectorates in India, he was enabled to lay before the English public, in a condensed form, the substance of those returns. Unfortunately it was necessary to have recourse to estimate in so many instances, that the results can only be looked upon as approximations to the truth, but they are the only approximations hitherto furnished from India.

Colonel Sykes states that sugar in India is produced from the sugar-cane and from trees of the palm tribe, the Date (*Phoenix dactylifera*), Palmyra (*Borassus flabelliformis*), and Cocoa-nut (*Cocos nucifera*), which trees give no inconsiderable return of sugar: for instance—

	Number of Date or other trees used for Sugar produce.	Produce in cwt. of Raw Sugar.	Equal to cwt. of Sugar.	Produce in lbs. per 100 trees.
Bengal Presidency.....	6,390,590	759,558	286,519	1330
North-western Provinces	258,071	470	156 $\frac{2}{3}$	270
Madras Presidency	6,468,368	484,838	161,612 $\frac{2}{3}$	838
Bombay Presidency	250,063	158	53 $\frac{1}{3}$	No average.

Note—In the N.W. provinces and in the Bombay presidency the juice of the date and other trees is chiefly used to make an intoxicating liquor.

Colonel Sykes then contrasted the returns from the several districts, and furnished the following analyses:—

	Acres under Sugar.	Produce tons of Sugar.	Date or Palm trees.	Produce tons of Sugar.	Total Sugar.	Consumption in India.	Available for Export.	Consumption of Sugar per head.	Produce per Acre.
Bengal	224,027	82,290	6,390,590	12,659 $\frac{1}{3}$	94,949 $\frac{1}{3}$	67,577 $\frac{1}{3}$	27,372	lbs. 3	cwt. lbs. 7 39
N.W. Provinces. }	610,179 $\frac{1}{2}$	128,077 $\frac{1}{2}$	258,071	8	128,085 $\frac{1}{3}$	72,644 $\frac{1}{3}$	55,441	8 $\frac{1}{4}$	4 12
Madras.....	28,316 $\frac{1}{3}$	13,104	6,468,368	7,883 $\frac{1}{2}$	20,987 $\frac{1}{2}$	12,711	8,276 $\frac{1}{2}$	2 $\frac{1}{3}$	9 26
Bombay ...	25,782	7,765 $\frac{2}{3}$	250,063	7 $\frac{1}{4}$	7,773 $\frac{1}{2}$	9,069	None.	8	6 2 $\frac{3}{4}$
Total	888,304 $\frac{1}{3}$	231,237	13,367,092	20,558 $\frac{1}{2}$	251,795 $\frac{1}{3}$	162,001 $\frac{2}{3}$	91,989 $\frac{1}{2}$		
Straits Settlements }	4,476 $\frac{1}{2}$	2,792	19,175	196	2,988	269	2,719	3 $\frac{2}{3}$	10 27

Colonel Sykes subsequently gave details of the cost of production of sugar in India.

Statistical Account of the Labouring Population inhabiting the Buildings at St. Pancras, erected by the Metropolitan Society for improving the Dwellings of the Poor. By Lieut.-Col. SYKES, F.R.S.

After stating what had previously been done by the Statistical Society of London and the Statistical Section of the British Association by way of inquiry into the domestic arrangements of the poor, Colonel Sykes said it was scarcely possible the reports made by these bodies, and read as they were at public meetings and subsequently printed, should fail to attract the notice of some of the many benevolent and practical men in England. Sir R. Howard, Lord Morpeth, Lord Ebrington, Mr. T. F. Gibson, Lord Grosvenor, Lord C. Hamilton, Mr. J. W. Tottie, and others, soon associated themselves together for the purpose of forming a "Metropolitan Association for the Improvement of the Dwellings of the Poor." A Royal Charter was obtained on the 16th of October 1845, limiting the profits to 5% per cent. In May 1848 the Directors reported the completion of the building, and that, of the 110 sets of rooms 103 were let; the number of applicants being 197, some of whom were refused for want

of proper reference. Up to that time they had not had a single default in payment of the rent, and general satisfaction was expressed by the tenants with the extra comforts and accommodation afforded them. The balance-sheet showed that the building had cost 13,252*l.* 17*s.* 11*d.* The last report is dated May 1849. It is the first occasion in which details of a year's occupation can be given, and they are satisfactory throughout. The directors say—"It affords your directors great satisfaction to state that all the dwellings have been occupied, almost without interruption, from the date of their completion, and several applicants have been and still are waiting for vacancies; 59 families have continued tenants since their respective dwellings were ready for occupation in January, February, March, and April 1848. The total number of tenants has been 173, several of whom having left their apartments have subsequently wished to return. Not only have the tenants expressed themselves pleased with the superior accommodation afforded to them, but have also proved, by regularly paying their rents, and their general strict observance of such rules as your Directors have thought proper to lay down for the management of so large a building, *that they are desirous of assisting them in preserving a high character for respectability in its occupants.*" Col. Sykes remarked that the last trait mentioned by the Directors is of extended bearing and importance; it holds out a prospect that not only will such communities be advanced in their physical and social condition, but that a feeling will originate within themselves to maintain a certain moral standing, a certain pride of character, which will prevent individuals or their neighbours in this community from offending against a public sentiment. The total number of shares taken at the date of the Report was 1527. The buildings up to that date had cost 17,225*l.* 5*s.* 3*d.* Colonel Sykes proceeded to consider how far, in addition to certain physical and economical advantages, this Association acts as an efficient auxiliary in the great efforts now making to improve the sanitary condition of towns. The best test, he says, for this would be the health of the population inhabiting the buildings of the Association; and he accordingly requested the Hon. Secretary, Mr. Gatliff, to have drawn out for him a weekly return for one year of the inhabitants, showing the male and female heads of families, children, weekly changes of population, number of deaths and previous occupation, age, and disease. The weekly outgoings and incomings rendered it a somewhat complicated matter to determine accurately the per-centage of deaths, and he consulted his friend Mr. Neison. The Return shows that there has not occurred a single case of cholera, although the fatal disease was all around the buildings.

Having viewed this picture in detail, in which a population is represented as comfortably housed, with the proper accompaniments of ventilation, proper supply of water and cleanliness, let us turn, Col. Sykes says, to a state of things the contrast of this picture:—In December 1847, a Committee of the Statistical Society of London inspected the dwellings, room by room, and condition of the inhabitants of Church Lane, St. Giles's, London. On the 17th of January, 1848, their report was made to the Statistical Society:—Church Lane was 290 feet long, 20 feet wide, and contained 32 houses. The population examined was 463; the number of families 100, and the number of bedsteads amongst them 90. There was an average therefore of above 5 souls to a bed, and many rooms were inhabited by as many as 22 souls, without water, without drainage, and without privies. The whole condition of these people was so revolting, that the Committee concluded their Report in the following terms:—"Your Committee have thus given a picture in detail of human wretchedness, filth, and brutal degradation, the chief features of which are a disgrace to a civilized country, and which your Committee have reason to fear, from letters that have appeared in the public journals, is but the type of the miserable condition of masses of the community, whether located in the small, ill-ventilated rooms of manufacturing towns, or in many of the cottages of the agricultural peasantry. In these wretched dwellings, all ages and both sexes, fathers and daughters, mothers and sons, grown up brothers and sisters, stranger-adult males and females, and swarms of children, the sick, the dying, and the dead, are herded together with a proximity and mutual pressure which brutes would resist; where it is physically impossible to preserve the ordinary decencies of life; where all sense of propriety and self-respect must be lost, to be replaced only by a recklessness of demeanour which necessarily results from vitiated minds; and yet with many of the young, brought up in such hot-beds of mental pestilence, the hopeless, but benevolent attempt is making to implant, by

the point passes several times over different parts of the same line of letters, an exact copy of the written communication is made, the letters appearing of a light colour on a background of closely-drawn spiral lines. There are numerous electrolytes adapted to mark the paper, but the one that has hitherto been found most available is the prussiate of potash dissolved in a diluted acid. Very complicated chemical actions ensue when the electric current passes from a steel point through paper saturated with such a solution, the result of which is to leave a stain of prussian blue on the paper. When the paper is moistened with diluted acid alone, the message is impressed invisibly on the paper, and it is brought out by afterwards passing the paper through a solution of prussiate of potass. By the arrangements described copies of writing may be made at any distance to which an electric current can be conveyed, provided the two instruments are moving exactly together. One of the alleged advantages of this telegraph, as compared with needle telegraphs, is that it will be free from the perturbing influence of atmospheric electricity. By another invention connected with the copying telegraph, independent connections with different stations and with branch lines may be obtained. The cost of the instrument would be 30*l*.

On the Britannia Bridge.

The President (Mr. R. Stephenson, M.P.) gave an account of the causes which produced the late accident, and of the difficulties which have stood in the way of finally completing the work.

On a Machine for Ventilating Coal Mines. By WILLIAM BRUNTON, C.E.

Although this paper referred to rarefaction as a means of ventilation, and not to the manner in which the air is coursed or conducted through a colliery, the author briefly noticed the principle upon which the air is conducted in the best ventilated collieries, that is, by dividing the workings of the colliery into districts, so that in no case does the air traverse through the whole of the workings, but being apportioned by the wasteman to each of the districts through regulating doors, and passing through the internal ramifications, the air of each district is ultimately and separately discharged into the waste or return air-course which conveys it to the upcast shaft.

As the tendency of the current is to take the shortest course, it becomes necessary to retard the current of the shortest, or first district, in order to ventilate the second, and the second to ventilate the third; and, in short, it is by judicious retardation that the air is made to pass efficiently through all, and more especially through the last or longest district.

Though the division of a colliery into districts is found, upon the whole, a very great improvement upon the original method of coursing all the air through the whole extent of the works, yet it is manifest that it has rather increased the necessity for care, skill, and constant supervision on the part of the wasteman, and under particular circumstances presents less security against explosion.

The author described the furnace (the ordinary power of rarefaction) for which the machine he has invented is intended to be a substitute, and proceeded to point out wherein that mode of rarefaction is defective and ill-adapted to the purpose.

Having thus endeavoured to state the nature of the furnace, and also what he conceives to be its inherent defects, he entered upon the description of the apparatus which he has invented and applied as a substitute. He drew attention to the principle of its action, namely, centrifugal force, and also to its construction—an integral drum, with radial or curvilinear compartments supported and revolving upon a vertical axis, whereby the air contained in the compartments is discharged with a force (and a corresponding measure of rarefaction is produced in the central part of the drum) due to the velocity with which it revolves.

Its construction is of the most simple character; it has no valves or separate moving parts, has no attrition, and all the friction is resolved into a foot pivot moving in oil: when at rest, it presents no impediment to the air ascending the pit, is very inexpensive, and liable to no derangement. In short, it is a simple integral implement, whereby any degree of rarefaction necessary to the ventilation of a colliery is rendered certain and regular, under all the changes that so injuriously

affect the furnace, and subject to visible inspection on the surface by the constant application of the water-gauge.

The machine erected by Mr. Powell, at Gelly Gaer Colliery, was for the express purpose of ascertaining its power of rarefaction and general applicability to the purpose of mine ventilation, in order to its application to his larger and more extensive collieries. The trial has been made, and a letter expressive of his entire satisfaction was produced.

The machine is applied to the colliery by an air culvert connected with the upcast shaft, which of course is closed at the top. The diameter of the drum is 22 feet, the radial length of the compartments 6 feet, leaving the central space 10 feet diameter: 16 feet being the mean diameter of the radial compartments, the centrifugal force at 120 revolutions per minute is 39.25, which, multiplied by the weight of 6 cubic feet of air = $\frac{444}{1000}$ of a pound, will give or produce a rarefaction to 17.5 lbs. on the square foot in the upcast shaft. The following table expresses the rarefaction consequent upon the velocities stated:—

	lbs.	Water in inches.	Mercury in inches.
60 revolutions per minute	4.3	per square foot 0.81	
90 " "	9.7	"	.13
120 " "	17.3	"	.24
150 " "	27.0	"	.38
180 " "	39.0	"	.55
210 " "	53.0	"	.73

These, though calculated as above, are most satisfactorily corroborated by actual experiments.

It will be necessary to draw attention to two conditions in which this machine may be placed; first, to that when unconnected with any pit or air course, viz. air from the atmosphere having free access to the centre and space for free discharge from the periphery, and a velocity given to it of 130 revolutions per minute, creating a rarefaction of 17 lbs. per square foot in the middle of the drum, then the velocity of the air through the machine would be 108 feet per second, and the aggregate quantity discharged 8424 cubic feet per second, or 505.440 feet per minute: in this (first) condition, the whole power is absorbed in displacement of air. The second condition is the very reverse of the first, viz. when no air is permitted to enter the central part, and of course none can be discharged at the circumference; the steam-engine exerting the same force as before, thus relieved from the resistance of the air passing through the machine, expends its power in increasing the velocity of the drum, thereby creating in this case an extreme or maximum effect in rarefaction. A due consideration of these opposite conditions proves that the resistance of the machine, or the power required to turn it, will be in proportion to the quantity of air ascending the upcast shaft, and the amount of rarefaction required to draw it through the colliery; and the principle of self-adjustment being such, that if from any cause a less quantity of air is passed through the colliery, the steam-engine (exerting the same power) will of itself accelerate the velocity of the drum, and thereby increase the rarefaction. For the power applied being the same, the effect will be commensurate, in the quantity of air discharged, the amount of rarefaction attained, or in a compound of both.

In contrasting this machine with the furnace, the author first notices, that it is applicable to any depth of shaft.

That it is not sensibly affected by change of atmospheric temperature or the fall of the barometer, but, on the other hand, by an increase of velocity at such seasons, may be made to obviate or counteract the danger connected therewith.

The uniformity of rarefaction maintained will also produce an uniformity of ventilation through the respective districts of the colliery, such as cannot be effected by furnace ventilation.

There will be no necessity for a stone drift to avoid the ignition of gases by the furnace.

The injurious effect of heated air upon iron in the upcast shaft will be prevented entirely.

It has been objected that machinery of any kind, from its liability to fracture or derangement, is inappropriate to the ventilation of a colliery; but this broad objection is daily overruled. To machinery, in a much more complex form, we commit ourselves with confidence to be carried by land at the rate of fifty miles per hour, or conveyed across the Atlantic. But where this objection may yet prevail, an additional or spare cylinder, with its appendages, may be attached to the opposite end of the frame ready to be applied if necessary. A suspension of the revolving of the ventilator would not be attended with immediate danger, for it offers no resistance to the ascending current, which would continue long enough for the men to withdraw from the pit, and repairs would be performed with less loss of time than is the case in the repair of furnaces.

With respect to cost, a ventilating machine with steam-engine complete, suited to an extensive colliery, can be supplied, erected, and put to work for about £350; and where there are suitable boilers already at the colliery from which steam can be obtained, the cost would little exceed half the above sum. Lastly, with respect to the power of rarefaction, the maximum effect of the furnace under the most favourable circumstances gives $12\frac{1}{2}$, whilst with the apparatus at Gelly Gaer 24 lbs. on the square foot has been produced.

But beyond the mere substitute for the furnace as applied to all the ordinary purposes of ventilation, the ventilating machine possesses advantages to which the furnace has no applicability. One of these is, that in the event of an explosion, the machine being on the surface, and placed with respect to the upcast shaft out of the reach of injury, can be immediately applied to clearing the pit of the choke damp consequent on explosion, and thus save the lives of men which would otherwise be lost.

Another, and what the author esteems by far the most important peculiarity possessed by the machine is, that it has such power of rarefaction that the atmosphere of a colliery may be subjected in half an hour to an artificial exhaustion of three-, four- or five-tenths of an inch of mercury, producing in the colliery, during the absence of the workmen and their lights, the very same exudation of the gases that would have taken place during the natural change of the atmosphere indicated by a like fall of the barometrical column; and before the men re-enter the mine the machine will discharge the noxious gas by a current of fresh air more copious and effective than can be produced by any other means in use. All that is needful to effect this is, upon the retirement of the workmen and their lights that the air be prevented entering the workings from the downcast shaft; the exhaustion alluded to will immediately commence, for the quantity of air ascending the upcast shaft being decreased, the drum will be accelerated, and the whole extent of the workings will thus be subjected in a few minutes to the full measure of rarefaction obtained in the upcast shaft; upon the fresh air being permitted to enter, the colliery will be found in a state of extraordinary purity of atmosphere, and freedom from the risk of explosion.

It is the concurrent testimony of all intelligent underground men, that the fire-damp exudes copiously during the fall of the barometer, and also that during its rise the reverse takes place*; the fissures that during the fall were discharging gas, now absorb or draw in atmospheric air; but the effects attendant upon a fall of the barometer must necessarily be more or less dangerous, in proportion to the time it has been rising or nearly stationary, when a large portion of the gas evolved during that period will have accumulated in the goaf-basins or vaults.

From these observations it is obvious, that if the fire-damp be drawn off at short intervals, as at every twenty-four hours, the accumulation and consequent danger will be very little, compared with what it frequently is through the continuance of weeks of fine weather; and the daily discharge of these minor accumulations will maintain the colliery whilst the men are at work in that state of safety experienced whilst the barometer is rising (see Messrs. Lyell and Faraday on the Goaf, p. 17).

Possessing thus the power of anticipating the sudden exudation of gas by drawing it off when it can do no harm, and of rendering the colliery much more safe and healthy for the workmen, may we not reasonably hope that the subject will receive the attention it deserves, and that a system of alternate exhaustion and restoration

* This remark must not be applied to fire damp, which issues from whole coal *under pressure*.

will be judiciously brought into practice as experience will dictate, until the Davy lamp shall be no longer necessary for the common collier, the danger of explosion almost or altogether obviated, and the health of the miner greatly promoted?

On the Use of Rockets in effecting a Communication with Stranded Vessels.
By A. G. CARTE.

On a Desiccating Process. By ROBERT DAVISON, C.E.

All previous modes of drying which have been employed consist in generating heat by simple radiation, or throwing off heat from a heated surface, whether the surface be brick flues, cockles, steam or hot-water pipes. Heat is easily attainable in this way, and almost to any grade of temperature, but heat is not the only essential for drying. Heat facilitates the evaporation of the watery particles, but a current is necessary; otherwise all the water which is thus converted into vapour will only tend to charge the chamber with steam, and it is not until the steam has arrived at a certain excess or pressure that it will make its escape and the operation of drying really commence. The amount of current obtainable in this way is proportioned to the rarefaction and quantity of air admitted and allowed to come in contact with the heated medium.

The paper then proceeds to show that it is not only a moving, but a rapid current which is the great desideratum for all drying purposes, and that it is the impulsion of atmospheric air at the velocity of the hurricane, or 120 feet per second, or any increased speed, combined with elementary heat under perfect control, which constitutes the desiccating process.

The means by which the two operations of current and heat are created and kept up are as follows:—The apparatus consists of a series of cast-iron pipes of a semi-circular form, so united together with straight pipes at their base as to form one continuous pipe; these being set in brickwork, with a common furnace in the centre, comprehend the heating medium. The current is created by a common blowing-fan, which can of course be driven at any required speed. It therefore only remains to be observed on this head, that the chief difficulty has been to discover what amount of surface or metal was necessary to secure or maintain a certain temperature in a given space: this has been found to be 28 feet superficial, or about 10 cwt. of metal for every 1000 feet of space of chamber.

In speaking of the application of the process to purifying brewers' casks, the author mentions, as a proof of the success of this portion of the invention, that upwards of one million casks have been thus cleansed and purified, and that the cost does not exceed 1½d. each cask; whereas any other method where unheading is resorted to, the cost is 6d. at least. Messrs. Guinness of Dublin are mentioned as having adopted the process.

The paper proceeds to describe the theory upon which *dry heat* is more important than moist or steam heat for this purpose, and also why water in the pores of the wood, and acid from the beer, are direct antagonists, quoting on this subject the experiments of Mons. Dutrochet, who discovered that one drop of acid in one ounce of water became mouldy in eight days.

In speaking of the application of the process to the seasoning of wood, the author alludes to the numberless treatises and cures for what is termed *dry rot*, but which a writer says is a misnomer, "*the rotting principle being moisture*," and asserts his belief that all immersions in water or exposing the wood to steam is calculated not only to dissolve and wash away the gums which nature has provided to bind the fibres together, but that all such methods have a tendency to sodden and decompose the woody fibre itself; while, on the other hand, the desiccating process is attended with an opposite result. The improvements are thus described:—

1. The process is a true imitator of nature, or those elements which are acknowledged to be the best seasoners of wood, viz. the March wind and summer heat, with the advantage of both being continuous and controllable until every grain or atom of moisture is expelled.

2. The greener the wood the easier and more perfect the expulsion of the watery particles.

3. The native strength of the fibre secured by the immediate evaporation of all vegetable juices likely to ferment and carry on decomposition.

4. The gums, instead of being removed, are coagulated and hardened, and the texture of the wood generally having been brought into its most complete state of aggregation and density, is much less liable to imbibe atmospheric moisture, and altogether less prone to decay.

5. The colour of mahogany and other fancy woods is not only preserved but improved.

6. Shrinkage is entirely obviated.

7. Cost of desiccating less than the interest of money on the value of wood, laying up to season in the ordinary way.

8. Inferior woods made useful and equally durable as the more expensive, all the chief elements of decay and mischief being expelled.

In general, there is the absolute certainty to the consumer that wood so treated is thoroughly dry, a matter which, according to the old system, is one of great doubt.

The author adduces proof from experiments, and exhibits specimens of the success of the process in the above particulars.

Two tables are appended, one showing the average per-centage of moisture removed from 100 different specimens of wood, which was $21\frac{1}{2}$ per cent., the average time occupied in desiccating the same being thirty-six days, together with the ratio of time in which equal degrees of desiccation were effected by the natural and artificial processes, which were as 40 to 1, the thickness being 1 to 12 inches.

A table giving the result of a few of the above specimens with their duplicates, which were afterwards submitted to a strain until they broke, the weight, deflection and average increase of strength by desiccating being noted, the additional strength being—

On yellow pine.....	17.6 per cent.
On Riga pine	20.4 ...
On mahogany	12.4 ...
On English oak	14.0 ...

Reference is made to the system of impregnating wood with preservative mixtures, and the author explains that where such is deemed desirable, that the most effectual mode of accomplishing the object is first to desiccate the timber, and immediately on its removal from the heated chamber to plunge it into any of the mixtures referred to in a cold state. But the author is of opinion, that by removing all the vegetable juices or elements of decay, there is little occasion for impregnating the wood with any foreign matter.

The author appends notices of the mode of conveying the currents of heated air into drying chambers generally; the application of the process to purposes where a high degree of heat is necessary; and to the roasting of coffee.

The paper proceeds at some length to describe the imperfections which exist in the ordinary modes of roasting, and states that in the desiccating method, intended to remedy these defects, the coffee is put into cylinders perforated throughout, or into cylinders made of gauze wire; currents of hot air are impelled into the midst of the berries, thereby driving off through an aperture in the cover of the apparatus "all the first water," to use a phrase, as in boiling potatoes.

The pernicious watery vapour being fairly expelled, the operator proceeds to adjust both the admission- and escape-valves, by which means the roasting is perfected in (to all intents and purposes) a close vessel, thereby retaining all the really essential flavouring properties of the coffee.

As a proof of the charring and deteriorating effect of coffee roasted solely by a highly heated metallic surface, 112 lbs. of raw coffee seldom produces more than 91 lbs., or at most 92 lbs. of roasted coffee, whereas the new mode has been found to produce, on an average of six months' working, 93 lbs. 6 oz.

On the Manufacture of the Finer Irons and Steels, as applied to Gun-Barrels, Swords, and Railway Axles. By W. GREENER, Birmingham.

The first innovation on the old principle of manufacturing gun-barrels entirely from old horse-nail stubs was due to the late Mr. Adams of Wednesbury, who

brought out what is termed Damascus iron, which is constructed of alternate layers of steel and iron faggoted, drawn down into rods, then tortuously twisted, and when welded into barrels forms the Damascus barrel. The success of this experiment, both in point of beauty and strength, was so great, as to be under-estimated at 50 per cent. as compared with the strength of stub twist iron. The next experiment was to blend more intimately than the above steel with the horse-nail stubs in the proportion of one to two of the latter. The paper described the mode of this; and then went on to narrate that the next and most important improvement in metals was the manufacture of gun-barrels from scrap steel entirely, and for this purpose old coach springs were generally in request: by clipping these into pieces, perfectly cleansing them, and welding in an air-furnace, a metal is produced which surpasses in tenacity, tenuity and density any fibrous metal ever before produced. The tenacity of it when subjected to torsion in a chain-testing machine is as 8 to $2\frac{1}{2}$ over that of the old stub twist mixture. The perfect safety of barrels produced from it is astonishing; no gunpowder yet tried has power to burst them when properly manufactured. These experiments had induced others on a more extensive scale; to effect this, ingots of cast steel were taken to the mill made to No. 3 in the scale of carbonization. These, after rolling into flat bars, were clipped into small pieces, immediately mixed and welded as before in the air-furnace, drawn down into rolls, and re faggoted; these were subsequently drawn down, and were then ready for being made into gun-barrels, either with or without spirally twisting them, to form Damascene barrels. It was discovered that the density and tenacity of the metal was sufficiently great to effectually resist the enormous force of the great charge of gunpowder. The manufacture of swords was another article to which this improvement applied. All the investigations of the writer had tended to satisfy him that the Arabs thus produced their finely-tempered Damascus swords; namely, using two steels of different carbonization, mixing them in the most intimate manner, and twisting them many fantastic ways, but observing method in that fancy; and it was a fact that no European sword has ever yet been produced equal to the Damascus. The Government inspector of small arms was of opinion that the swords made in Birmingham were not fit to be used in the army. The writer's investigations had satisfied him that tempering by crystallizing the steel, that is to say, tempering in the ordinary way, was far from the wisest. The Damascus blade in its fibrous state or hammer hardened is more difficult to break by 100 per cent. than the best English-made blade. This had been tried; but temper it the same way, and it showed no greater tenacity than our own; the Damascene figure was destroyed by the carbon becoming equally diffused; nor would acid develop it—it was entirely gone. From these and other facts the conclusion might be drawn, that swords constructed of dissimilar steels—tempered by condensation of its fibres, either by repeated rollings, hammering, or many other processes, which our perfect machinery gave us the facility to do—are the best. Therefore in time we might hope to see every soldier of the empire armed with a weapon as good if not so costly as the highly-prized Damascene. The remaining part of the paper referred to a subject already much discussed—the manufacture of railway axles.

On the Cause and Prevention of the Oscillation of Locomotive Engines on Railways. By GEORGE HEATON, Birmingham.

The author exhibited a model to show the cause and prevention of the oscillation of locomotive engines upon railways, made to a scale of $\frac{1}{4}$, to represent a locomotive engine with driving wheels 6 feet diameter, cylinders 16 inches diameter; stroke of piston 22 inches. A handle in connexion with the machine turns once round for 20 strokes of the piston. When the handle is turned slowly round, the machine stands still where it is placed; but by increasing the speed, it will begin to oscillate and jump about, although each wheel has a balance-weight in it equal to the weight of the crank-pin and the weight that the connecting rod bears upon the crank-pin.

If weights are placed on the wheels equal to the weight of the pistons and gearing, or all that moves in a horizontal line, the oscillations will cease, but the machine will begin to jump perpendicularly up and down.

This machine is intended to show the importance of moving weights in opposite

directions to each other at high velocities. It will be found when an engine of 20-inch strokes with 6 feet driving-wheels goes 15 strokes per minute or 3 miles per hour, it requires one-tenth of the weight moving along the horizontal line (that is, the pistons and gearing moving backwards and forwards within the engine-framing) to stop it and turn it again.

At 35 strokes per minute, or about 7 miles per hour, one-half its weight; at 74 strokes per minute, or about 15 miles per hour, $1\frac{1}{2}$ time its weight; at 100 strokes per minute, or about 20 miles per hour, four times its own weight will be required for the same purpose. By attaching a weight with connecting rod and an auxiliary crank to the head of the crank-pin, equal to the piston and its gearing, so as to make the weight run to the left-hand at the same instant the piston goes to the right, the blow to stop the piston and make it return at each end of the stroke will be received in the auxiliary crank instead of in the wheels, producing a neutral point in the centre and steadiness of motion; for when the blow is received in the wheels, the cranks being at right angles, it is communicated through the axle and gives a twisting motion to the whole framing of the engine; this being repeated with regularity, produces an effect similar to the rocking of a boat. This oscillation is found to be greatest when the engine is running most regular for speed and the piston going the same way with the oscillation of the carriage. The wire screwed to one of the piston frames with a loose piece of iron upon it, is intended to show at what speed the wire can move backward and forward before it begins to slip. If a weight be moved in a similar manner with a 20-inch stroke, it will slip at 35 strokes per minute, or less than 2 feet in a second; therefore, when it is considered that the pistons of locomotives frequently travel at the rate of 18 feet in a second, or come to a dead stop and turn again 10 times in a second, it must be evident that the blow is sufficient to make the whole engine oscillate and jump in a most fearful manner; when it is also considered that an engine running at 100 strokes per minute, or the pistons and gearing travelling at about $5\frac{1}{2}$ feet per second, require four times their own weight to stop them and turn them again, when the weight of each piston, &c. frequently exceeds 400 pounds.

To prove that the steam has no action in causing the oscillation, but merely to blow the different bodies apart, I bored a small hole in the middle of a gun-barrel, and placed 2 pieces of iron weighing 20 ounces each, which fitted the bore of the barrel up to each side of the hole. I then put a few grains of gunpowder between them, the barrel being placed upon a bed of sand to show if it moved, and then fired the gunpowder; the pieces of iron were each blown the same distance from the hole, and the barrel remained where it was placed.

Experiment 2. I then placed 10 and 20 oz. in the same position as before, and fired, the barrel remaining still; the 10 oz. weight was blown 4 times the distance of the 20 oz.

Experiment 3. I then placed 5 oz. and 30 oz. in the same position and fired it, the barrel remaining still, but the 5 oz. weight was blown 40 times the distance of the 30 oz.

The oscillations of the machine do not increase in violence regularly as the speed increases, for if the pistons do not keep time with the oscillations of the machine, it is then tremulous motion until the pistons go with the machine every second stroke, and so on to any speed, similar to a railway train that will shake violently when running along a straight level line at about 23 or 24 miles per hour; increase the speed 2 or 3 miles per hour, it will become nearly steady; by further increasing the speed the oscillation becomes again violent, just as the whole weight of the engine can oscillate in proportion to the weight of the piston and its gearing compared with the whole weight moved.

In making some experiments with this machine I raised 10 cwt. 16 feet high, and attached it to the shaft the handle is upon, and then let it run down; it came to the floor in 242 seconds. I then attached the auxiliary crank with connecting rod, balance weight, &c., and raised the weight to the same height as before; it came to the floor in 210 seconds, although adding so many more moving joints the friction of which had to be overcome.

On the Strength and Elasticity of Stone and Timber.
By Professor E. HODGKINSON, F.R.S.

This was the result of a series of carefully conducted experiments on the power of stone, timber and other bodies, to resist tension, compression and transverse force. The experiments have long been in progress, and will be very numerous and varied when completed; they will include the strength of most of the stones used in architecture in this country, and of thirteen kinds of timber. The results of some of them were laid before the Association, at the Manchester Meeting in 1840; but as they are incomplete, the author is desirous of giving no more at present than this notice of them, except to state that they point to some important general conclusions.

On a Calculating Instrument. By H. KNIGHT.

The inventor of the machines exhibited is a Mr. Slonimski of Byalystock in Poland. The first instrument submitted to the notice of the Section was one for performing the arithmetical processes of addition and of subtraction. It consists of a thin box of wood or metal, covered by a plate of metal, in which are perforated a convenient number of circular apertures and openings, around which are engraved or marked the several figures or digits 0 to 9, and behind which are indented plates or wheels, having in each a suitable number of teeth, some of which are shaded, or black, the others being left clear, or white. A small pointer, or style, is furnished with the instrument, for the purpose of turning round the indented plates or wheels, by inserting the instrument between two contiguous teeth, and moving it in the required direction. The style is required to be inserted between those two teeth which appear under the particular figure engraved on the plate, which corresponds with the number required to be added or subtracted; one general rule being to be attended to, viz. that if the style be placed between two clear or white teeth, it must be turned to the extreme right-hand of the circular opening; but if between two dark teeth, it must be turned to the extreme left-hand thereof. The upper part of the instrument is to be used for addition, and the lower part for subtraction, as engraved thereon. The multiplication instrument consists of a rectangular box, about 15 in. square and 3 in. deep. It contains cylinders having printed tables of figures on the circumference of each, which cylinders revolve separately, by means of the knobs at the bottom of the box; and by other knobs, the upper part of each cylinder is moveable in a vertical direction also, the rotative and the vertical motions being regulated by figures termed indices, that appear through small holes over the axes of the cylinders. In addition to the index holes, there are nine other rows of holes, on the surface-plate of the instrument, the lower row of holes being for the multiplicand, and the corresponding rows of holes above it to exhibit the products of that multiplicand by each of the nine digits; and these products are produced almost immediately, and without requiring any mental effort. The horizontal number of holes in this instrument is eight; and it is therefore calculated to give the product of any number having seven places of figures, or to millions, whatever may be the order of those figures. This instrument is the result of a new theorem of figures discovered by M. Slonimski.

On a New Rotary Engine. By the Rev. J. W. M'GAULEY, Professor of Natural Philosophy to the Board of National Education, Ireland, &c.

The following are the facts, &c. deduced from a series of experiments, made by myself and the Messrs. Allingham of Dublin, conjointly. The species of engine to which they have reference is exemplified by the model, now working before the Section.

No one knew better than Watt the excellence of the ordinary engine, and yet he was extremely anxious to construct a *rotary* engine: indeed he, with this object,

patented contrivances, which, had they been successful, would not have been remarkable for simplicity.

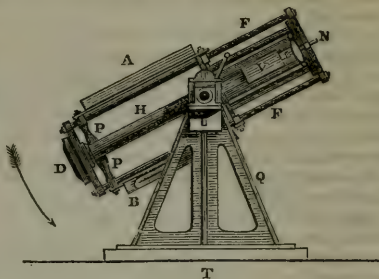
The chief inconveniences attending the use of the ordinary engine are, first, the loss of power arising from the *obliquity* of the connecting rod; and, secondly, that which is the necessary consequence of bringing large masses of matter alternately into a state of rest and motion, many times in a minute.—A great waste of power must follow from the obliquity of the connecting rod, since it is a fundamental principle in mechanics, that “power is lost when the force which we have, and that which we require, do not act in precisely the same direction.” The oscillating engine obviates, to a greater or less extent, the inconvenience arising from *obliquity*; but it increases, to a very serious amount, the quantity of power consumed in stopping and moving large masses of matter. The seriousness of such an evil was perceived very clearly by the experiments witnessed in this Section, on Friday morning last, regarding the oscillations produced in locomotive engines, by the alternate motion of the piston and the parts in connection with it. The ingenious contrivance, on that occasion brought before the meeting, was admirably calculated to prevent the oscillation; but, so far from diminishing the loss of power, due to the bringing into alternate rest and motion the reciprocating parts of the machine, it adds to the evil by increasing the weight of these parts. Loss of power and oscillation at high velocities, are far from being the only inconveniences which follow from this cause.—With powerful engines, intended to move machinery, there is an irregularity of motion arising from it, which, though not perceptible in ordinary circumstances, becomes exceedingly injurious, where regularity is—as in many manufactures—of deep importance. The wear and tear, also, consequent on great strains in opposite directions, is very considerable.

Rotary engines may be conveniently divided into two species: in one of them the steam produces, *at once*, a rotary motion; in the other, it produces that motion *indirectly*, by means of the crank, or some such contrivance. As to any of those engines which produce a rotary motion *directly*, I cannot but feel convinced, that, however ingenious they may be, or however smoothly they may work, from the very nature of things, the most simple of them must be far more difficult to construct than the ordinary engine; and, if it is at all possible to keep them steam-tight, it can be effected only by a complication of parts, which renders them greatly inferior to reciprocating engines, whatever their defects may be. Undoubtedly many such rotary engines are extremely beautiful, and, externally, appear sufficiently simple; but they depend too much, in general, on accuracy of original construction, and do not fulfil a condition which I believe indispensable to a good engine, which is, that “the more it works, within reasonable limits, the better and more steam-tight it will be.” This condition, I may venture to assert, belongs in the fullest degree to the engine now brought before the Section.

While most persons admit that the ordinary engine has considerable imperfections, all are not agreed as to the causes whence they arise. Many, confusing the facts connected with the *crank*, fall into errors regarding it; some concluding that because, as a *lever*, it does not destroy power, its application to the steam-engine is attended with no loss; while others infer that because its use is *accompanied* by a loss of power, it therefore itself destroys power. Neither opinion is correct: and I am even free to admit that the engine now before you contains, in principle, a crank—which, as we are taught by the very first principles of mechanics, during every part of its revolution, gives back, though *modified*, all the force communicated to it. We, therefore, did not propose to ourselves to obviate the necessity of using a crank, but to change the mode of its application, since to this is due the loss of power arising from its use.

None being more convinced than we of the excellent qualities of the ordinary engine, our whole object was to retain its good points, while we endeavoured to exclude its defects. Nothing can be more simple nor effective than the ordinary piston, cylinder, slide valve, &c.: we have retained them. It is highly advantageous to work the steam expansively: we effect this as easily, and I think I could show to a greater extent, than can be done by the ordinary engine, and by a contrivance which is completely under our control.

It is seen, by the model at present working, that we effect our object by using *gravitation*, as a medium for obtaining the power of the steam. Weights A and B are raised twice every revolution, through a space equal to the stroke; and in falling, give back the force which raised them but little diminished. D is a counterpoise to the cylinder N, and the parts connected with it. The cylinder, piston and rod, steam-chest V, &c., are like those of the ordinary engine; the whole revolves on standards, one of which is seen at Q, bolted on a bed-plate T. The axle turns



in brasses, which are in the upper part of the standards, and are quite independent of the valves, placed on the extremities of the axle, for the purpose of admitting the steam or carrying off the waste: these valves work against metallic plates, into which are screwed respectively the steam and waste pipes, and which are bolted to, and adjustable on shelves cast upon the standards. The weights are attached to the cross-head of the piston-rod, which, as well as eyes fixed to the other extremities of the weights, slide upon the guide-rods F and F. So little power is wasted by this engine, that we found $82\frac{1}{2}$ to represent the effective force, the whole power of the steam being considered as 84. The action of the steam is precisely in the required direction, at whatever part of the revolution the weights may be raised.

The plan we have endeavoured to carry out has, no doubt, occurred to many persons long since; but we have reason, from our own experiments, to conclude that it was most probably abandoned, whenever tried, on account of difficulties which would scarcely have been anticipated, at least to their full extent; and which would have discouraged us, were we not determined to persevere, as long as any prospect of success should remain. To overcome these difficulties cost us more than three years of labour and the expenditure of several hundred pounds.—One of the chief of them was the stopping, with sufficient gentleness, those masses of matter contained in the weights. The moving parts of the ordinary engine are gradually set in motion or brought to rest, chiefly by means of the crank; and whatever consumption of steam or loss of power occurs, is nearly, if not entirely, concealed from the ordinary observer. But, in our case, if the reciprocating parts were not, with gentleness and certainty, brought into a state of rest, the engine would be greatly injured, or destroyed. The weights, which are thrown up with very considerable velocity, cannot be stopped by solid or unyielding matter, since it would cause the most injurious concussions. On the small scale, springs might be used for the purpose, although any person who makes the experiment will find, that while they prevent concussion they cause a very objectionable amount of vibration: on the large scale, they would be quite inadmissible. But, as is perceived by the model now working, so gently are the weights moved and stopped by the means we employ, that not the *slightest* noise, concussion, or vibration is produced. And I pledge myself that increasing the size does not diminish these advantages. I work almost every day, and often for many hours a day, an engine which, among others, I constructed myself upon this principle. Its weight amounts to about 160 lbs., and it makes ordinarily 120 revolutions per minute, that is, 160 lbs. are stopped and put in motion 240 times per minute; and yet this produces no more noise nor vibration than is perceived with the smallest model. It is so free from any kind of strain, that I habitually work it at its highest velocity, while merely *resting* on its bearings—there being nothing whatever pressing down on the axle to keep it in its place. My friend Mr. John J. Allingham has now nearly finished an engine which may be worked up to eight-horse power, and I am perfectly satisfied that it will revolve with the same gentleness and silence.

It might at first be supposed, that we should require large and clumsy weights:—

the contrary will, however, be evident to any one who takes the trouble to make the necessary calculations. The relative size of the weights diminishes as the engine becomes large.

Since the whole engine acts as a fly, we require no fly-wheel, and there is no temptation to make any portion weak; since the heavier and stronger it is, the greater its power. Its entire weight may be less than that of the fly-wheel of an ordinary engine of the same power, and it will not occupy more space than would be required for the fly-wheel only.

It might be thought that such large masses, at one side of the centre of motion, must necessarily cause great strain and vibration. But I beg to call attention to the important difference there is between the uncounterbalanced masses being *moved by* the power, and being themselves *the power*. If they are moved by the power they will cause a very great strain and vibration, as might be illustrated by fixing even a small fraction of them to a wheel having the same diameter as the circle, in which their centre gravity revolves when they are attached to the engine.—We soon found it necessary to counterbalance the cylinder, &c. by the counterpoise D H, which at the same time, serves also as a strong and convenient support, along with the cylinder, for the guide-bars F, F. On the other hand, the weights themselves, however great, being the medium through which the force is transmitted, require no counterpoise.

Since the steam is used very expansively, the upward motion of the weights is gradually retarded, and any small quantity of it, which remaining would cause concussion or vibration, is destroyed by a portion of the waste steam. There is not, however, any obstacle offered to the exit of the latter, which has, at least, as great facility for escape as in the ordinary engine; and whatever quantity of it acts as a cushion, is so much towards the filling of the cylinder at the next stroke.

We do not require a governor, since the engine contains within itself more than one principle of self-regulation, which prevents its speed being sensibly altered, even with extreme variations of resistance.

It might be supposed that we lose power by *centrifugal force*, but this is not the case, since whatever extra force may be necessary, on account of it, during one part of the stroke, is given out during the other. It affords us one principle of self-regulation; for if the engine even slightly exceeds the proper speed, the increased centrifugal force causes the weights to be projected upwards with increased velocity; but the waste not having then sufficient time to escape, the stroke is shortened, and by consequence the velocity is diminished. A little consideration will show that the consumption of steam is exactly proportioned to the length of stroke, and therefore to the power of the engine.

I may take this opportunity of remarking, that the *rotary motion* is another source of self-regulation; for it not only, by means of a *fixed* excentric, opens the ports at the proper time after their having been closed by the upward motion of the weights, but also, by cutting off the steam more rapidly, and diminishing the velocity with which the waste escapes, shortens the stroke, and thus reduces the speed of revolution.

The slide-valve has *two* motions quite independent of each other, and yet the contrivances by which they are effected are, to a certain degree, connected together. The *fixed excentric* opens the ports exactly at the proper time, whenever that may be; and the upward motion of the weights, in whatever position raised, closes them to the required extent. These movements, though, as is perceived by the model, so simple and effective, are the result of long and troublesome experiments.

The *overrunning* of the piston at high velocities, was, at first, one of our great difficulties, but it is now highly advantageous to us. The piston, at high velocities, moves so rapidly, that the steam, not having time to follow it, is used still more expansively than in other circumstances.

The weights are, no doubt, *falling bodies*, but our velocity is not on that account limited, since they are in reality bodies which *continually* fall, and therefore have their velocity increased till it reaches a maximum, dependent on circumstances.

The portions of the weights which are at one side of the centre of motion do not counterbalance those at the other.—The *common centre of gravity* of the reciprocating masses is lifted through a distance equal to the stroke, and before it can be

again raised by the steam, must, in falling, have given back the force which raised it, minus that which is destroyed by friction at the axle, and which, from the latter merely *resting*, as we have seen, *on* its bearings, must be inconsideable.

At first we experienced much inconvenience from the difficulty of getting the steam conveniently in and out of the axle, since we were not satisfied with the arrangements employed with the oscillating engine. But we found that this difficulty might be removed, by a valve of great simplicity—and which has now been, for a considerable time, working most effectively.—It has but little friction, because it is exposed to equal and opposite pressures, and its rubbing surfaces are metallic.

This engine works equally well in either direction, and may be reversed without turning off the steam, by means of a handle—seen in the figure over the standard Q, and a little to the right-hand.

The details may be varied in a great many ways. The weights may be placed respectively at the extremities of the piston-rod, trunnions being cast or bolted on the centre of the cylinder. The weights themselves may act as cylinders, the steam being transmitted through hollow piston-rods, fixed to the hollow axle. Also a single cylinder, moving backwards and forwards on a hollow piston-rod, having the piston in its centre,—and a variety of other modifications may be employed; but the form exemplified by the model working at this moment before the Section, has the advantage of resembling, in almost all its details, the simplest form of the ordinary engine.

On an Instrument called the "Upton Draining Tool," as illustrating a principle by which the resistance of Soils to Agricultural Implements may be considerably diminished. By A. MILWARD.

When we consider the rudeness of the agricultural implements in actual use over a great part of the country, and the want of fixed principles in the construction of the more complicated kinds, it may not perhaps be altogether useless to draw attention to a general cause of resistance to the efficient working of agricultural tools, and to show how, in the use of a particular tool, that cause of resistance has been very materially diminished.

It is not until very recently that the accurate researches of science have been brought to bear on the practical operations of agriculture. The services of chemistry have, it is true, been great; the experiments and energy of Mr. Josiah Parkes and others have thrown considerable light on the action of water upon soils, and thence established certain principles of draining; and skilful mechanics have materially improved our old implements, and invented many others of very great utility. But much, nevertheless, remains to be done in determining the nature and extent of some of those natural causes which it is the business of the farmer to overcome or turn to his own advantage. The laws of capillary attraction,—the porosity and fineness of division of different kinds of matter, particularly as connected with the former subject,—the phenomena of friction and adhesion,—have been so little investigated, that it is impossible to apply them with any exactness, or draw satisfactory conclusions from them. All these subjects are nevertheless intimately connected with the processes of agriculture; but it is unfortunately but too true that the ignorance of practical men, and the indifference of men of science, leave them to undeserved neglect.

The cause of resistance, to which I have alluded above, is what I may distinguish by the name of *after resistance*, by which I mean resistance arising from the friction of the *after* parts of an instrument when dragged through or over the ground. If we take the case of the common spade, we find that it requires very little force to cause it to enter an inch into a stiff soil; but as it descends, the closing of the elastic soil around it occasions so great a friction and adhesion along the after part of the tool, that a strong man is soon unable to drive it further. If, on the contrary, the spade be driven in on the side of an open cutting, the slice of earth cut off falls forward, the after resistance is much diminished, and the facility of working sufficiently marked. This simple consideration points out to us the utility of providing that the fore-part of any instrument for penetrating the soil shall as much as possible *detach* the soil from the parts around, and so prevent their closing upon the tool and

increasing the after-resistance. The different forms of ploughs, many of which violate this principle, illustrate a remark previously made, that too little attention has been bestowed on the reasons for agricultural practice. The peculiarity of running the share of the plough several inches beyond the coulter evidently occasions a loss of power, because that projecting portion is pressed on every side by the undivided soil. It is true that an advantage is claimed on the ground of the plough working more steadily and not tilting up; but that advantage might be secured without so unnecessary a waste of power. Again, the varying degree of polish on different forms of ploughshares shows a variation in the resistance accompanying those forms, but which has by no means been subjected to careful investigation. In the case of mould-boards something has been done; but I am not aware that even this subject has been treated in a scientific manner, although eminently suggestive of such treatment.

The Upton draining tool, it will be perceived, is trough-shaped, or consisting of two sides united at an angle of 60° , so that its section would be like the letter V. The spit of earth taken out by such a tool is evidently an equilateral prism, and such a prism is one of those regular solids which will fill up space, so that the length of a drain may be marked out into successive prismatic spits of earth fitting against one another. This consideration suggested the peculiar use of the tool, which I will presently describe.

Previous draining implements have consisted of two kinds, according as the blade is plain or curved, and with all of them it has been considered necessary to excavate the upper part of the drain 10 or 12 inches wide, when a narrower tool is substituted for the requisite depth beneath. The width actually *necessary* at the bottom of a drain for the insertion of draining pipes does not exceed 3 inches, so that the greater width at the top, if it can be avoided, is only a waste of time and labour.

It will be found that in the practical use of both the flat and curved tools three thrusts are made by the workman for every spit of earth withdrawn. In a clay soil, the adhesion of the spit of earth to the sides of the drain is too strong to allow of its being forced out without the assistance of lateral thrusts to detach it from the sides. The workman therefore uses his tool as is represented in plan in fig. 1, where the black lines show the marks on the surface which are made by the insertion of the tool, and the Nos. 1, 2, 3 indicate the spits of earth thus successively removed. What is here done by *three* thrusts the instrument now described effects by *one*.

Fig. 1.

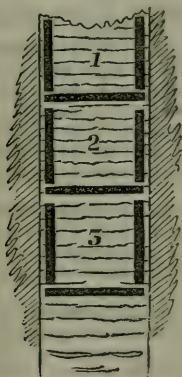
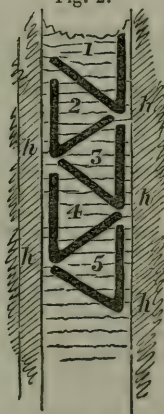


Fig. 2.



The manner in which the instrument is used is represented in plan by fig. 2. Here, as in the preceding figure, the black lines represent the marks made by the insertion of the tool; and it will be seen that the right and left sides of the tool are placed

alternately against the right and left sides of the drain, the letter *h* indicating the position of the handle.

The larger tool, the blade of which is 20 or 22 inches in length, will excavate the drain to the depth of about 20 to 22 inches, and the smaller tool, which is usually longer, will at one stroke increase that depth to $3\frac{1}{2}$ feet, or even more. I have had the opportunity of seeing a drain excavated by these tools, which was only $4\frac{1}{2}$ inches wide at the top, 3 inches at the bottom, and upwards of 5 feet deep. The soil was a stiff loam and free from stones, which is necessary to the advantageous use of such narrow excavations.

When a commencement has once been made, the facility of working is remarkable; the tool is pressed down, and the spit of earth detached, with much less exertion of force than in the case of any other tools. This arises from the spit of earth being completely separated from the surrounding soil as the tool descends, so that the after-resistance is avoided; and it is only the fore-part of the tool which has an obstacle to overcome. On examining tools which have been some time in use, I have found the varnish still remaining after the first few inches, showing that the upper or after part was but little subjected to friction.

A little dexterity is requisite in keeping the tool properly along the side of the drain, and also in withdrawing the spit of earth when cut out. On the latter account the tool must be inclined from the perpendicular, as in the case of other draining tools, and not thrust straight down into the ground. On account of the diminished resistance, the size and weight of most other draining tools is rendered unnecessary, and the handles may be constructed of a lighter and cheaper wood.

The tool which has been described is the invention of one of the Life-members of the Association, who was led to its adoption from a consideration of the advantages to be gained by attending to the principle before alluded to, and from a practical acquaintance with the subject of draining in a country where the soil is comparatively free from stones.

On an Oil Test. By JAMES NASMYTH.

The author has contrived an instrument by means of which the comparative value of various kinds of oil, as lubricating agents, may be ascertained. In all the contrivances which have been proposed as oil tests, a most important element has been left out, viz. *time*, inasmuch as the evil which is experienced from the use of a bad quality of oil is only developed after the lapse of several days, when, by the action of the oil upon the metal with which it is in contact, together with the action of the air, such oils become viscid, and begin to clog instead of facilitating the movements of the parts of the machinery it was intended to lubricate.

In the more delicate descriptions of machinery, such as chronometers, watches, clocks, &c., such a defect as the thickening of the oil by lapse of time is a most serious evil; and in examining into the comparative fitness of certain oils for such applications, if we do not include *time* as an element in our examination, we shall be led to form most false conclusions, inasmuch as it is the case that for the first day or two some kinds of oil (linseed oil for example) perform the lubricating duty very well, but at the end of the second or third day they become so thick and viscid as to entirely arrest the motion of the machinery.

The most valuable quality in an oil intended for the lubrication of machinery is *permanent fluidity*. That oil which will for the greatest length of time remain fluid in contact with the iron or brass is without doubt the most useful for the purpose. Hence, as before said, the necessity of including the element of *time* in any experiment on the comparative value of such oils.

Some idea may be formed of the importance of having the means of arriving at correct conclusions on this subject, when we know that in some spinning establishments there are upwards of 50,000 spindles in motion at the rate of 4000 or 5000 revolutions per minute! The slightest defect in the quality of the oil in such a case, by its becoming viscid, tells in the most serious way upon the quantity of fuel consumed in generating the power required to maintain at this high velocity such a multitude of moving parts.

The slight increase of fluidity consequent on the rise of temperature, caused by the

lighting of the gas in the rooms of a cotton-mill, makes a difference of several horse power in the duty of the engine of an extensive establishment.

The oil test consists, as will be seen, of a plate of iron 4 inches wide by 6 feet long, on the upper surface of which six equal-sized grooves are planed. This plate is placed in an inclining position, say 1 inch in 6 feet. The mode of using it is as follows:—Suppose we have six varieties of oil to test, and we are desirous to know which of them will for the longest time retain its fluidity when in contact with iron and exposed to the action of the air; all we have to do is to pour out *simultaneously* at the upper end of each inclined groove an equal quantity of each of the oils under examination. This is very conveniently and correctly done by means of a row of small brass tubes. The six oils then make a fair start on their race down hill; some get ahead the first day, and some keep ahead the second and third day, but on the fourth or fifth day the truth begins to come out; the bad oils, whatever good progress they may have made at the outset, come soon to a stand-still by their gradual coagulation, while the good oil holds on its course, and at the end of eight or ten days there is no doubt left as to which is the best; it speaks for itself, having distanced its competitors by a long way. Linseed oil, which makes capital progress *the first day*, is set fast after having travelled 18 inches, while second-class sperm beats first-class or refined (?) sperm by 14 inches in nine days, having traversed in that time 5 feet 8 inches down the hill. The annexed table will show the state of the oil race after a nine days' run.

Results of Oil Test.

Description of oil.	First day.		Second day.		Third day.		Fourth day.		Fifth day.		Sixth day.		Seventh day.		Eighth day.		Ninth day.	
	ft.	in.	ft.	in.	ft.	in.	ft.	in.	ft.	in.	ft.	in.	ft.	in.	ft.	in.	ft.	in.
Best sperm oil ...	2	8 $\frac{1}{2}$	4	2	4	5 $\frac{3}{4}$	4	6	4	6	4	6	4	6 $\frac{1}{8}$	Stationary.		ft. in.	
Common sperm oil	1	7	3	9	4	6 $\frac{3}{4}$	4	11	5	1 $\frac{1}{2}$	5	4	5	6 $\frac{3}{4}$	5	7 $\frac{3}{8}$	5	8
Galipoli oil	10 $\frac{1}{4}$	1	2 $\frac{1}{4}$	1	6	1	6 $\frac{1}{2}$	1	7	1	8 $\frac{3}{4}$	1	9	1	9 $\frac{1}{4}$	1	9 $\frac{1}{2}$	
Lard oil	10 $\frac{1}{4}$		10 $\frac{3}{4}$		10 $\frac{3}{4}$		10 $\frac{3}{4}$		11 $\frac{3}{4}$	Stationary.								
Rape oil	1	2 $\frac{1}{2}$	1	6 $\frac{1}{4}$	1	7	1	7 $\frac{1}{8}$	1	7 $\frac{1}{4}$	1	7 $\frac{1}{4}$	1	7 $\frac{1}{4}$	1	7 $\frac{3}{4}$	Stationary.	
Linseed oil	1	5 $\frac{1}{2}$	1	6	1	6 $\frac{1}{4}$	1	6 $\frac{1}{4}$	1	6 $\frac{1}{4}$	1	6 $\frac{1}{4}$	1	6 $\frac{1}{4}$	Stationary.			

Mr. Nasmyth feels satisfied that this mode of testing the qualities of oil intended for the lubrication of machinery will give out much useful information, and therefore trusts it will meet with attention.

On Gordon's Plan of Ventilating Coal Mines. By WM. NICHOLSON.

The essential principle adopted in this plan is to cover over the upcast shaft, and to conduct the foul air in it to a chimney rising above the surface of the ground, and having one or more furnaces at its base. In particular cases it is recommended to close also the top of the downcast for a short time at night, so as to compel an *exhaustion* of foul air from the recesses of the works. The author regards the position of the furnace which he has chosen, at the surface, as superior on many accounts, to that commonly in use at the base of the upcast shaft.

[Complete drawings accompanied this communication, and explanations in detail of several points which would not be understood without diagrams.]

On a Water Meter. By W. PARKINSON.

A cheap, simple and accurate method of measuring water as it flows from a reservoir or other source of supply, and registering on an index the proper quantity passing through such machine, has long been a desideratum. There are, however, many difficulties to contend with, such as the great and variable pressure used at different works; and in addition to this pressure, the effect produced by suddenly stopping any current passing through the meter, which would require very great strength of material to resist; another difficulty is the variable speeds produced by variable elevations of water, which would often drive the meter beyond a proper speed, and quickly derange and make it imperfect; another, the difficulty of making

it susceptible of small quantities passing through it under light pressure; this may be illustrated by what takes place in actual practice in dry meters, and shows the difference between the wet and the dry meter. The wet meter, a simple wheel or drum revolving partially in water, without a click, clack or valve; its passages are alternately sealed or immersed in water, preventing the possibility of escape of gas without being registered. The dry meter consists of diaphragms, valves, levers, cranks, stuffing-boxes, &c., and may be compared to two or three steam-engines, the only difference being the diaphragms instead of cylinders and pistons. In practice, as may be expected, the valves become defective, and oftentimes allow large quantities of gas to pass unregistered, and at all times small quantities, unless when new and perfectly clean and oiled; whereas the wet meter, until actually worn out by time or corrosion, continues to register every particle of gas passing through it. These two descriptions, therefore, will serve to illustrate the two kinds of water meters—the one consisting of valves, pistons, &c., the other without, as brought before the notice of the British Association by the patentee, and in which the pressure is put under perfect control before the water enters the body of the meter, and so limits the supply to a maximum quantity, and thus gets rid of the evil of being over-driven and derangement. The next question is, how to get a supply to the upper rooms of a house. No difficulty can occur here if the pressure of water be always on and sufficient to reach such elevation; there can be no necessity to measure the water before it ascends; it is only necessary to place the meter at the highest point; the water will of course descend by its own gravity, and supply every room or closet below, thus ridding this meter of the difficulties which the other had to contend with, and reducing it to one of easy construction, comparative cheapness, and not easily deranged. Its durability may be calculated to be as great as that of the gas-meter, which often goes twenty-five and thirty years without repair. A greater durability in this may be looked for, from its always working in pure water, and from its extreme simplicity; the measuring-wheel or drum being similar to that in the gas-meter, the regulating apparatus which reduces the pressure to the proper level, at fixed periods, being accurately measured, and all the parts self-acting; and it is supposed that now Water Companies have a fair chance to get paid for all they sell, it will be to their interest to keep up an uninterrupted supply; although this does not prevent the old cistern being supplied by meter, it is evident that a larger meter would be required. By the new system, the whole apparatus may very conveniently be placed inside the house free from the effects of frost, the annoyance from this and short supplies; to these advantages may be added the assurance of justice and equity between buyer and seller.

The Sheet-Metal Moulding Machine.
By RICHARD ROBERTS of Manchester.

This machine, of which a drawing, on a scale of one and a half inch to a foot, was shown, is furnished with two shafts, which project beyond one of the side-frames, in which the lower shaft turns; the upper shaft is mounted on a balance swing-frame, and is connected by spur gear with the lower shaft, in such a way that the distance between them may be adjusted to any required extent, without altering the depth of the wheels in gear. On the projecting ends of these shafts are put the rollers with which the mouldings are to be formed. The lower roller is generally in one piece only; but the upper roller is made in one or more parts transversely, as may be best adapted to form the required mouldings. These parts are made to approach each other, by being slid along the shaft, which is hollow, by means of a screw within, that acts upon the back part of the top roller, by means of a cotter that passes through the shaft and the screw, and on the front part by means of a nut, which is screwed from time to time by hand.

The machine is furnished with a third roller, by which it is adapted for giving any degree of curvature to the mouldings. For some mouldings, two or more pairs of rollers are used in succession. When two pairs are used, the first pair is to crease the metal preparatory to its being formed into the required moulding by the second pair of rollers.

In another drawing, sections of a railway bar or rail, in two stages of manufac-

ture from the "pile" of iron, were shown. The finished bar differed in shape from that called the "bridge" form, inasmuch that it had additional flanches on the inner side, giving it the appearance of a square tube. It was suggested that deep, narrow girders might be rolled with facility on the above plan.

On correct Sizing of Toothed Wheels and Pinions.

By RICHARD ROBERTS *of Manchester.*

Although much has been written on this subject, and on the best form of teeth, there is still much difference of opinion on both points, which difference is not confined to individuals, as it embraces the members of the trade or profession to which they belong; for instance, engineers, millwrights, and machinists in general, adopt the plan of extending the teeth of the pinion and wheel to the same distance beyond the pitch-line, and the majority of them are agreed as to the best form of teeth, namely, the cycloid for wheels to work in straight racks, and the epicycloid for wheels to work in other wheels or in pinions; they are not, however, so well agreed respecting the length of the teeth; whilst the makers of watches, clocks and chronometers, do not extend the teeth of the pinion, beyond the pitch-line, more than one-half as far as they do the teeth of the wheel: hence the preference given by those trades to the "bay-leaf" form for the teeth of the pinion, as no other form would pitch with their sizing.

Various rules are given in works on horology for sizing wheels and pinions; but I believe movement makers generally, English and foreign, use for that purpose an instrument (called a sector) resembling a two-foot rule, which is divided into equal parts throughout its length, commencing about half a division from the centre of the joint. The numbers up to 10 or 12 on the sector are usually subdivided, for the use of artisans who may prefer pinions a little larger than the corresponding number on the sector would give.

Knowing it to be essential to the correct performance of any machine where wheels and pinions are employed that they should be properly sized, I have thought it might be useful to parties whose experience on the subject has been more limited than my own, to be informed respecting the plan which I have adopted for more than thirty years for sizing toothed wheels. It has long been the practice in Manchester to make those wheels which come under the denomination of clockwork with a definite number of teeth to the inch diameter, taken at the pitch-line, and to distinguish the pitch accordingly. I have done this, and have adopted the same plan in respect to mill-gear, substituting the foot for the inch diameter in designating the pitch (naming the pitch by the number of teeth in a foot diameter), instead of naming it by the distance from the centre of one tooth to the centre of the next, which is, I believe, the universal practice. I may here mention, that, in the year 1816, I had a set of change-wheels made by a factory clockmaker, which were so much out of pitch as to direct my attention to the cause of the defect; and having found that the error had arisen from the defective principle of the sector, I immediately contrived a sector, which differed from the clockmaker's sector, principally in the joint being adjustable like that of the proportionable compasses. The use of this kind of joint was to enable parties to pitch wheels correctly, and to suit themselves as to the depth of the teeth. After I had sold a number of sectors of this kind, I found that for all ordinary purposes a fixed joint would answer equally well, provided the centre of the joint were equal to two of the divisions of the sector below zero. This circumstance led me to make sectors (if serrated bars of brass may be so called) of a cheaper kind, whilst they were better suited for the workshop. This kind of sector, which I believe is made by my firm only, has the tenth number at the twelfth division from the starting-point, the two divisions being added for the depth of tooth beyond the pitch-line. There are two scales on each of these sectors, one on each edge, which scales are marked according to the number of teeth to the inch diameter (pitch-line) they are adapted for. These sectors are made in sets, ranging from three to thirty in the inch. We use one set for sizing working-wheels, and another set for sizing pattern-wheels, the latter being one per cent. coarser than the former, to compensate for the contraction of the metal in cooling. In a patent which I recently took out for improve-

ments in watches, &c., I included a sector adapted for sizing small wheels and pinions, by which the calibre (distance between the centres) of the wheels may be found.

A specimen of the Genevese watch-sector, and one of each of the other sectors above referred to, were produced; also a jointed and a straight sector for sizing mill-gear, both of which had the allowance of two divisions for the length of the teeth beyond the pitch-line.

The Excentric Sheet-Metal and Wire-Gauge.

By RICHARD ROBERTS *of Manchester.*

A plate of brass, about four inches and five-eighths diameter, and a quarter of an inch in thickness, is recessed on the upper side to the depth of an eighth of an inch and diameter of four inches, leaving a margin of five-sixteenths of an inch broad. In the centre of the recess is a hole, into which is fitted a steel pivot, whose upper end is riveted into a steel disc three inches and eight-tenths diameter and one-sixteenth of an inch thick; the pivot is excentric to the disc one-tenth of an inch, and consequently one point in the periphery of the disc touches the inner edge of the brass margin, with which the top of the disc is level. To the under side of the brass plate a small slide is fitted, to the outer end of which is attached (by screws) a piece of steel that passes up through a notch in the brass margin about half an inch, and forms the inner or sliding jaw of the gauge; the outer jaw is formed of a similar piece of steel, also passed through the notch in the brass margin, and is attached to the brass plate by screws.

The inner edge of the sliding jaw is rounded to a radius of one-sixteenth of an inch, and is kept in contact with the periphery of the excentric disc by a spring (under the disc), which acts against a stud in the slide, projecting through the brass plate. The margin of the brass plate is divided through one-fourth its circumference, commencing at the centre of the sliding jaw, into seventy-five equal parts, which are numbered decimally. The extremity of the disc is then set at zero on the scale, and the jaws accurately adjusted to touch each other, after which the extremity of the disc is turned to the tenth division, and a line is made on the disc to correspond with zero on the scale, at which point the jaws will be open a little. The disc is turned to the required gauge-number by means of a milled button or two studs, and is fixed there by a milled nut, on the end of the pivot below.

It may be convenient to have the numbers extended from seventy-five on a fourth of the circumference to one hundred on a third; but the law of increase in the sizes above sixty-five would be reversed. It will be obvious that gauges having different numbers and dimensions may be more suitable for certain descriptions of work, and likewise that the excentric principle may be applied to gauges in various ways.

The excentric metal gauge possesses the following properties:—First. A corresponding gauge may be made, without expensive tools, from a written description of the means employed to make the original. Secondly. It admits of accurate construction and easy readjustment. Thirdly. Each succeeding number being larger than the preceding in a progressively increasing ratio, adapts the gauge equally well for high and low numbers.

N.B. The excentric gauge is not recommended for the workshop, but as a standard to make and test shop-gauges by.

Mr. Richard Roberts of Manchester described, by reference to drawings, his patent apparatus, by which the influx and efflux of the tide are rendered available as agents for effecting (by hoisting weights for that purpose) the motions of clockwork; thus, on a shaft, in connection with the clockwork, or other apparatus requiring motive power, a chain-wheel (No. 1), provided with studs or projections to prevent the chain slipping, is made fast; on a second shaft, which may be in line with the former one, is a similar chain-wheel (No. 2), also made fast; over each of the chain-wheels one side of an endless chain is passed, so as to form a loop on each side the shafts; from a pulley in one of the loops formed by the chain the going-weight is suspended, and from a pulley in the other loop is suspended the counter-weight (the going-weight being heavy enough to actuate the clockwork and raise the counter-weight), by which arrangement, if the second shaft be kept stationary, the clock-

work will be actuated until the going-weight be run down, to prevent which taking place, the influx and efflux of the tide are, by means of the following mechanism, made to rewind the going-weight. Loose on the second shaft are two other chain-wheels, provided with clicks, which take alternately into ratchet-wheels fast on the shaft. Over one of the last-mentioned chain-wheels a chain is passed, which, after passing under a pulley on a stud in the framing, is passed over the other chain-wheel, so that both ends of the chain hang on the same side of the shaft. To one end of the chain a weight is suspended, heavy enough to wind the going-weight before referred to; to the other end of the chain a hollow weight is attached, which is heavy enough to wind its counter-weight and the going-weight. The hollow weight is suspended in a tank, with which the tide has free ingress and egress, except as hereinafter explained.

The action of the apparatus is thus:—On the flow of the tide the water rises in the hollow tank and buoys up the hollow weight, which operation allows the weight at the other end of the chain to descend, and that weight, by means of the chain-wheel (No. 3) and ratchet-wheel and click above referred to, turns the second shaft and chain-wheel (No. 2), and thereby winds the going-weight. With the efflux of the tide, the hollow weight descends and rewinds its counter-weight, and at the same time, by means of the chain-wheel (No. 4) and ratchet-wheel and click, turns the second shaft, which again rewinds the going-weight by means of the chain-wheel (No. 2).

As during the descent of the going-weight and ascent of the counter-weight, the length, and consequently weight, of the chain actuating the clockwork continually varies, another chain of equal weight per foot is attached, by its extremities, to the going- and counter-weights, below which it extends, beyond the range of the weights; consequently, as the going- and counter-weights descend or ascend, the weight of chain is diminished as much at one end of the weights as it is increased at the other; by which means the effect of the apparatus is rendered equimotive.

As the level of high- and low-water will vary considerably at different seasons, it is evident that some limitation of the height to which spring tides would raise the going-weight must be effected, inasmuch as, unless this were done, the ebbing of those tides would cause it to be overwound, and the machinery damaged. To prevent which the action of the winding apparatus is limited thus: on the orifice through which communication is made between the tide and the water in the tank, a valve opening inwardly is placed, which is connected by a cord or chain to one arm of a lever, whose fulcrum is above the tank, and on the other arm of which a weight, just sufficient to keep the valve open, is placed; the weighted arm of the lever is then attached, by means of a cord (or chain) to the lower end of the going-weight, the length of the cord being adjusted to cause the going-weight to lift the weighted arm of the lever and close the valve before the ebbing tide overwinds the going-weight, the descent of which, by the going of the clock or other mechanism, will open the valve previous to the returning flow of the tide.

On the Superiority of Macadamized Roads for Streets of large Towns.

By J. PIGOTT SMITH, Birmingham.

There is a prevalent feeling against the employment of broken stone roads for streets, because, as they are usually managed, they are the cause of great inconvenience to householders and others by the dirt and dust they occasion, and also because their maintenance and repairs are very expensive, while the draught of vehicles upon them is very heavy. The object of this paper is to prove, from long-continued experience on a large scale, that those objections do not necessarily accompany the use of such roads. In discussing this question the interests of two parties must be considered: those who principally use the road,—the owners and employers of horses and vehicles,—and those who pay for it,—the rate-payers, the parties who would be injured and annoyed if it were unduly expensive or unnecessarily dirty, dusty and noisy. It is a common error to consider that road the cheapest which costs the least in direct expenditure. If, however, this so-called cheapest road causes waste of horse-power, undue wear and tear of horses and vehicles, loss of time by being unfit for rapid transit, and occasions loss to the inhabitants by filling their

dwellings with dust and covering their clothes with dirt, it is evident that such a road is really very dear. There is an apparent diversity of interest between those who use and those who pay for our public streets; as the principal loss from bad roads falls directly upon those who keep or employ horses and vehicles, while the expense of road repairs falls upon the inhabitants generally. A little consideration, however, will show that this diversity of interest is more apparent than real. It is the interest of all that there should be easy, safe and cheap means of transit through the public streets; and any increase in the cost of transit is a source of indirect expense even to those who have no horses of their own, as it must add to the cost of everything carried through the streets, and of all hired vehicles, and of all the numberless conveniences which accompany residence in a large town. It must also be remembered that it is very wasteful to allow a road to go out of repair, since it is less costly to keep a road up than to restore it. That roadway is best for the owner or user of a horse or vehicle which can be travelled over most easily, safely, quickly, and cheaply; and that ease, safety, speed and economy are to be obtained by having the road firm, even and smooth, and perfectly free from mud or dust, or any form of unattached materials. It is evident that the same qualities will render the roadway most free from noise, dirt and dust, the three great causes of annoyance and injury to the inhabitants of all ordinary streets. The question which remains to be considered is, whether the advantages of good roads to the inhabitants generally are worth their cost? If the question had to be decided in accordance with the interest of the users and owners of horses merely, no doubt whatever would be entertained. Of whatever nature the surface of a road is to be, it is essential that its foundation should be of firm material, well consolidated, and perfectly drained; if not, the crust becomes loosened and destroyed, the road is rough and uneven, and wears into holes and ruts. Having obtained a good foundation, the next point is to cover it with a hard, compact crust, impervious to water, and laid to a proper cross section. The stones must be broken to one regular size, well raked in, and fixed by a binding composed of the grit collected in wet weather by the sweeping machines and preserved for this purpose. This binding must be laid on regularly, and watered until the new material is firmly set, which it will do very quickly and with the regularity of a well-laid pavement. The sharp angles of the stones are preserved, and there is both great saving of material and a firmer crust formed than by the common method of leaving the material to work into its place without the use of binding,—in which case the angles of the stones are worn off and reduced to powder, and at least one-third of the material is wasted in forming a binding in which the stones may set. By the improved method, the binding is formed of material that would otherwise be useless. Many road-makers object to the use of binding, on the ground that the road is rendered rotten by it, and that when the road is set it has to be carted away again. This is apt to be the case under bad management, and when ordinary soil is used, the fine particles of which work it into mud and keep the road from setting firmly. But the coarse grit obtained by the sweeping-machine off the roads is the very same material as is produced by wearing away the angles of the stones, and when judiciously applied to a new coating it will speedily become as well consolidated and firm as an old road. In the common method, not only is there great waste of material, but the loose stones occasion delay by their resistance, great fatigue to the horses and danger to their feet, while the noise produced by their grinding together is annoying to the inhabitants. Upon the improved method the inconveniences of road repair are incomparably less than those of pavement. Both recoating and repairs may be made without stopping the traffic. Under no circumstances must any imperfection of surface be allowed. If a hollow be not immediately stopped, it very quickly extends over the surface. All loose stones should be carefully picked, as every loose stone passed over by heavily laden carriages, if not ground to powder, breaks the crust of the road, and if water be permitted to lodge on the surface it will cause great mischief. It is the neglect of these essential precautions that has led many to consider macadamized roads expensive. They are expensive if neglected. On a well-made road heavy showers do good, by cleansing them; so also does artificial watering if the road be clean or swept quickly after it is watered. A road which is perfectly dry loses its tenacity and the surface grinds into dust; whence the economy of judicious watering in hot

weather, which preserves the road as well as prevents the annoyance of dust. The practice so common in London and elsewhere of heavy watering a dirty road without cleansing it, and thereby converting the dust into mud, is very injurious to the road, and merely changes one nuisance into another,—dust into mud. A great source of waste, both to those who use and to those who repair a road, is to allow it to be dirty. The draught on a dirty road is twice as heavy as on a clean one, that is, a horse must exert double force to draw his load with the same speed. The cost, however, of employing double force is so great, that the expedient of diminishing the speed is generally adopted, as a horse can exert greater pulling force at a slower pace,—less power being required to carry his own body. It often happens that the extra resistance occasioned by dirt diminishes the speed one-fifth or one-fourth. The effect of the dirt, therefore, is to increase the work by twenty or twenty-five per cent. It will easily be believed that such a waste far exceeds the cost of the most perfect cleansing. This is the case when cleansing is done by scrapers (the greatest enemy a macadamized road has to contend against). By their use the stones are dragged from their places, and the adhesive dirt is not effectually taken away. Sweeping is the only mode of cleansing that should be allowed, either on streets or on turnpike roads. Sweeping by the wide brooms of Mr. Whitworth's machine is preferable to all other modes of cleansing yet tried. It must be evident, that the fact of these wide brooms sweeping longitudinally, with a pressure that can be adjusted according to circumstances, tends powerfully to preserve the road and to consolidate its surface. They press most upon the ridges, and least upon the hollows, thus tending to reduce the former, and fill up the latter. When the dirt is stiff and adheres firmly to the stones, it should first be well-watered, when it may be completely removed by the machine, without disturbing the crust, leaving the surface firm and compact. The use of water for this purpose has been objected to by high authorities, on the ground that it *does* remove the useful grit; but the contrary has been proved by ample experience. I have found that the use of the sweeping-machines, with the proper employment of water, has reduced the amount of material required for the repair of roads in Birmingham one-third, namely, from about 20,000 to 13,000 cubic yards. The first-named amount is the average for seven years preceding the introduction of the machines, the latter of the three years subsequent. I communicated these details to a friend in London, and he determined to test their correctness. The following is the result of his experiment, to settle whether useful grit was or was not removed by water and machine-sweeping. On the 22nd of March last, the Quadrant, Regent Street, was covered with a thick layer of dirt, which was causing great annoyance as well as injury to the road, but could not be removed by scraping without removing also much of the new stone, to which it adhered. It was determined to sweep half of it dry, and half after proper watering. This was done, and the sweepings removed were washed, to separate the refuse from the stony matter mingled with it. One-third part of that which was taken dry consisted of coarse grit, which would have been useful on the road; one-twelfth part only of that which was removed in the form of slop was stony matter; and that was so completely pulverized as to be of scarcely any use; it had done its work. After the two portions of the road had been cleansed, the difference between them was very striking. That which was swept dry was still covered with adhesive matter, which was lifted by the wheels, together with the stones to which it adhered, the whole road being rough and uneven; the portion which had been swept with water was perfectly even and smooth. On the 24th both portions were swept, but only one quarter as much dirt was taken from that which had been water-swept as from the other. On the 26th it rained, and three times as much slop was taken off the part of the road which had not been water swept on the 22nd. The preservative effect of water machine-sweeping was most evident by the decidedly better condition of that portion of the road cleansed in this effective manner. The great objection urged against macadamized roads for streets is the annoyance by dust and dirt which they occasion, and many persons prefer submitting to the deafening noise of pavement in order to avoid these; but this would not be the case if water and machine-cleansing was adopted, the cost of which would be saved in diminished wear and tear. The entire cost of cleansing and watering Birmingham is about 5000*l.* per annum, or about one penny per week for each house, or half a farthing

per week for each of its inhabitants. It has been objected to macadamized roads that the draught upon them is heavier than upon pavement; and with carriages altogether similar this is the case, and especially so with vehicles travelling slowly. But it must be remembered that the proportion of the draught is only one of the circumstances by which the labour of the horse is to be estimated. Another very important consideration is the surface which gives the horse the safest footing; and his footing on pavement is so much less secure than upon a good broken stone road, that he does not receive the full advantage of the smaller draught. Again, carriages—especially those travelling quickly—are exposed to much more violent concussions upon pavement than upon a smooth macadamized road: consequently, not only must the carriages be stronger and therefore heavier, but the increased frequency and violence of the concussions consume a larger portion of power, which goes far to counterbalance the diminished friction. There can be no doubt that the wear and tear of both horses and vehicles is far greater upon pavement than upon macadamized roads. In reckoning the real cost of a road, all expenses attending its use should be calculated; and if this were done, pavement would be perceived to be exceedingly expensive. Carriages roll so smoothly over a well-maintained macadamized road, and horses are so little injured either by falls or strains, that I conceive the wear and tear upon them is not half of what it is on pavement.

On a Method of supplying the Boilers of Steam-Engines with Water.
By W. SYKES WARD.

Mr. Ward's suggestion was to use a small supplementary pumping-engine, having a working cylinder with valves so arranged that the piston may be put in motion by either steam or water passing through it, to be supplied with steam by a steam-pipe, the entrance to which is somewhat narrow, and inserted in the boiler to be supplied a little above the level at which it is desired to maintain the water therein. Such aperture should also be about the centre of a marine boiler. The working cylinder should be attached to a pump of such size as to be easily worked by the pressure of the steam. The exit-pipe of the steam-cylinder must communicate with the inlet-pipe of the pump, so that if the cylinder be actuated by steam, the steam will be condensed, and its heat communicated to the water to be supplied to the boiler; or if the working cylinder be worked by water proceeding from the boiler, a considerable part of such hot water will be returned by the pump. The mode of operation of such apparatus will be, that whenever there is a working pressure of steam in the boiler, the apparatus will be in action; but if the level of the water be below the aperture of the small steam-pipe, the action will be moderately rapid, and a supply of water be pumped into the boiler; and when the water in the boiler rises to the aperture, this being small, will be as though choked by the water, which will be forced through the working-cylinder, moving the piston and pump very slowly; a portion of the water thus escaping from the boiler will be returned by the pump. Such last-mentioned action cannot continue long, inasmuch as the level of the water must be reduced; therefore the average level of the water in the boiler will be, with slight oscillations, maintained at the height of the aperture of the steam-pipe.

On Chain Pipes for Subaqueous Telegraphs. By F. WHISHAW.

Three links of a full-sized pipe, for enclosing the wires of electric telegraphs in crossing rivers, &c., were laid before the Section. As the title implies, the pipe is formed by so many links connected together by sockets. Each link varies, according to circumstances, from 18 inches to 24 inches in length, and from 1 inch to 2½ inches internal diameter, according to the number of wires to be enclosed. These pipes, being of wrought iron, are exceedingly strong, and are required merely as a protection to the wires, which are previously insulated by means of gutta percha. Pipes of somewhat similar construction are laid under the Rhine and other rivers in Prussia, where the under-ground system of telegraph is adopted by the Prussian government (already to the extent of 1200 miles), although many of the railway companies suspend the wires between posts, as practised in England, America, France, &c.

On the Present State of Electro-Telegraphic Communication in England, Prussia, and America. By F. WHISHAW.

Mr. Whishaw stated that the object of his present communication was not to bring before the Section the numerous telegraphic instruments now in use and recently made public, but to point out the advantages and disadvantages of the three great systems of electric telegraphs now in operation in England, Prussia and America. In England, the wires, being suspended from post to post along the sides of railways, are exposed to the following disadvantages—running of trains off the lines, by which posts and wires are all carried away together, and thus the communication is stopped. Secondly, from atmospheric influences, whereby irregular and uncertain deflections of the needles in Cooke and Wheatstone's telegraphic instruments take place, besides occasional destruction to parts of the instruments, &c. Thirdly, from snow-storms, as in the case of the South-Eastern telegraph which occurred during the last winter, when the wires and posts were all removed, and considerable interruption was caused in the transmission of communications. Fourthly, from damage by malicious persons, who sometimes twist the wires together; and for whose apprehension rewards have frequently been offered by the English companies. Fifthly, the wires have sometimes been connected together by a fine wire nicely soldered to the line wires, and thus the communications have been diverted from their right channel. Sixthly, the expense, viz. 150*l.* a mile, for the above-ground system, with an annual expenditure for repairs. Seventhly, and consequently, heavy charges for the transmission of messages. Eighthly, the time required in learning perfectly the manipulations of the needle telegraph, so that if a telegraphist is from any cause disabled, there is no one at hand to take his place. With regard to the charges, the following facts will suffice to show the advantages of economical telegraphs. In America, the charge for twenty words transmitted by the telegraph to the distance of 500 miles is but 4*s.*; whereas by the English company's charges the same communication would only be transmitted 60 miles, or less than one-eighth the distance, and by the South-Eastern Company's charges not 20 miles, or one-twenty-fifth of the 500 miles. Again, a communication of ninety words in America may be transmitted from Washington to New Orleans, 1716 miles, for 41*s.* 8*d.*; whereas by the Electric Telegraph Company's charges it would only be transmitted a little more than 200 miles, and by the South-Eastern Company's scale under 100 miles. The extent of telegraphs in Great Britain at present is about 2000 miles; and there yet remain railways to an equal extent without telegraphs. Mr. Whishaw expressed a hope that within a short time every principal town in the kingdom would be connected by telegraph, as the underground system may be effected without the aid of railways, viz. under turnpike-roads and towing-paths, &c. This plan has been practically carried out in Prussia, where at the present time there are 319 German miles=1492.92 English miles in actual operation. A single wire coated with gutta percha is laid under the railway at a depth of two feet, and connected with the instruments and batteries at the different stations. A colloquial and also a printing telegraph are used in each principal station—both worked as required by the single wire. The experiment as to burying the gutta percha wire in the ground was commenced some years ago, and being found to answer perfectly, the Prussian Telegraph Commissioners appointed in 1844 determined on adopting the underground plan entirely for the government telegraphs, which were commenced in July 1848, so that no time has been lost in carrying them out. At Oderberg, the Prussian system is in connexion with the telegraphic line now in course of construction between that place and Trieste *via* Vienna; and as regards the Prussian Government Telegraphs, the public has the advantage of them by payment of certain fixed rates. The cost of the Prussian system is under 40*l.* a mile. The American system is remarkable for the great extent to which it is already carried, viz. 10,511 miles, costing less than 20*l.* a mile. It consists of a single iron wire supported from post to post, but is carried far beyond the limits of railways, and is consequently frequently damaged, so that a code of rules is established for the repair of the wires, which is undertaken by gentlemen living along the lines, and who are furnished with a set of tools for the purpose, their reward being the free use of the telegraphs for their own private communications. The economy of first cost, however, causes a very

low tariff for the transmission of communications, so that the poorest person is enabled for a few cents to send a communication to a considerable distance. From the actual operations of the three systems, it appears that the Prussian is the most simple, effective and economical, for annual repairs are not required to the line wires, as in the cases of England and America, where they are exposed to so many casualties.

On Kosman's Patent Cistern as a Sanitary Machine. By — Wood.

Mr. Wood observed that the great object of this apparatus was to cause a simultaneous flow of water from the respective houses of each street, by which a thorough cleansing from the source of deposits may be obtained. The cistern is so contrived that it may be caused by a self-acting valve to discharge a periodical flow of water through the drains of the house, which, combining with a similar simultaneous discharge from the other houses, would sweep the sewers clean every three or four days or more, as may be found desirable. The advantage is considered to be, that instead of stagnant collections of soil, we have a perfectly fluid and attenuated flow at short intervals, easily carried off by the waters of a river and leaving nothing behind. The cistern is divided into two unequal parts: the larger part for domestic purposes, the smaller part to discharge its contents into the drain suddenly and with great force through a large pipe, which operation is accomplished by the means of a chain which is attached to the arm of the ball and to the valve at the bottom of the cistern.

Woodcut omitted in Mr. LEA's paper 'On Traces of a Fossil Reptile,' see p. 56.



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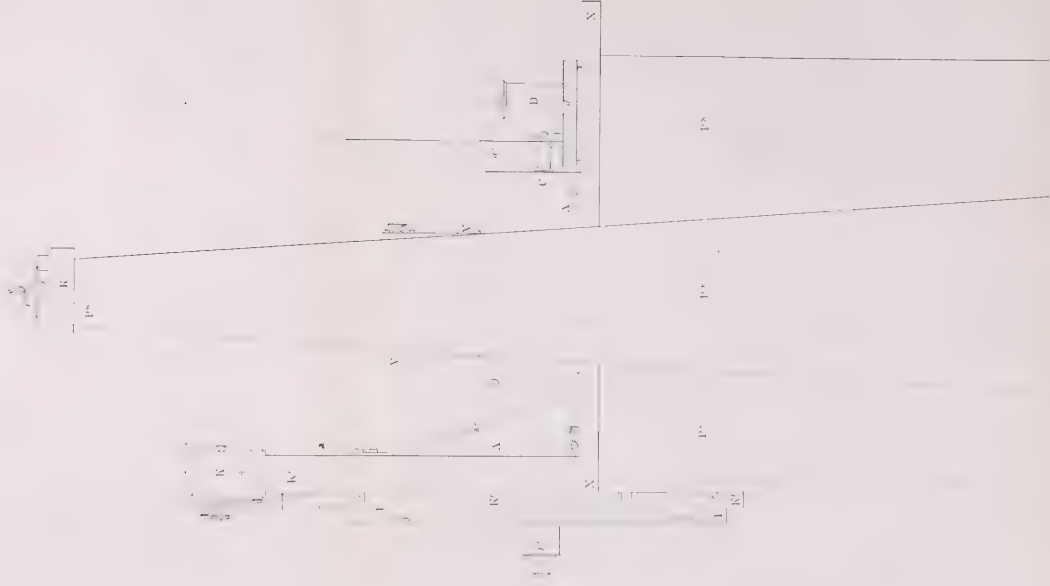
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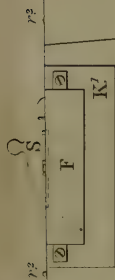
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THE END.

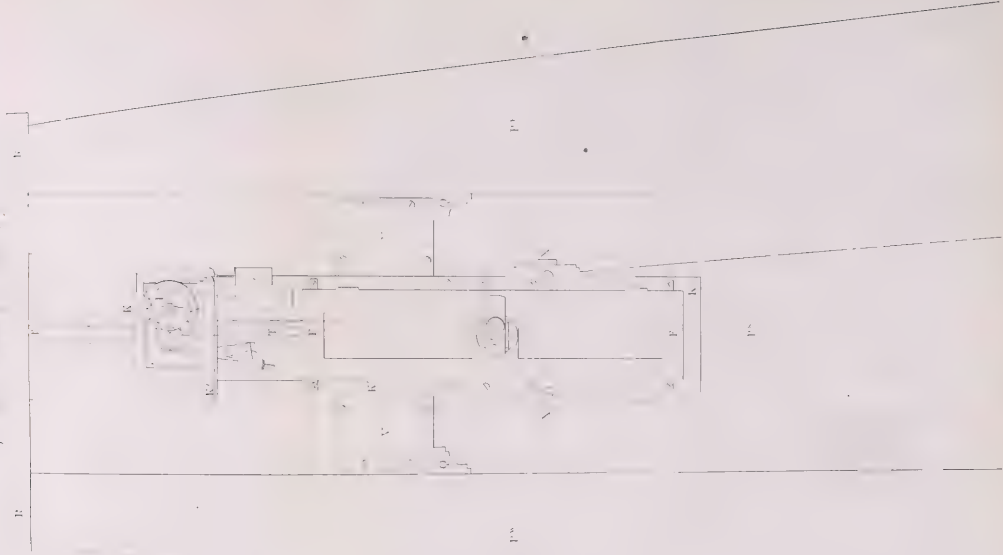
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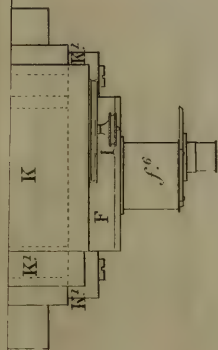
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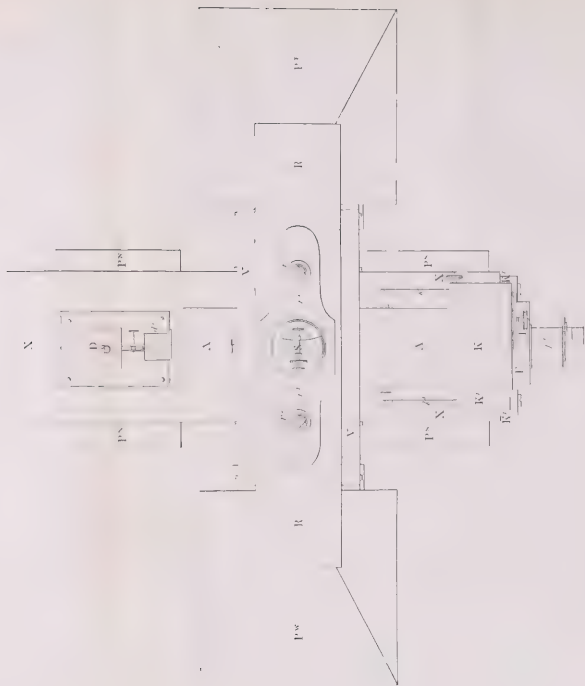




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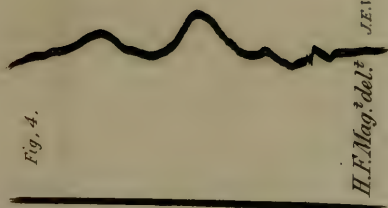
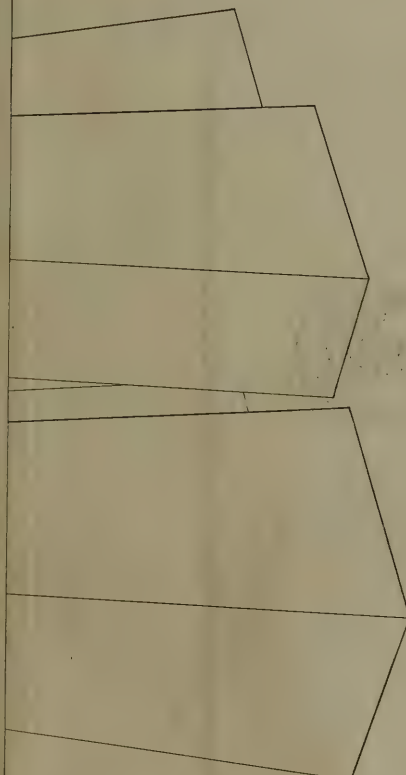


Fig. 4.

H.F. Mag. del.

J.E. Wood, Sc.

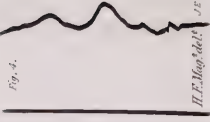
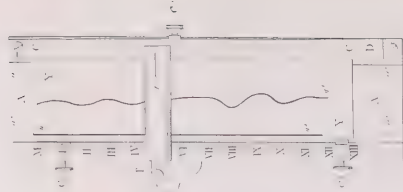
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J. E. Wood, Sc.



Fig 1

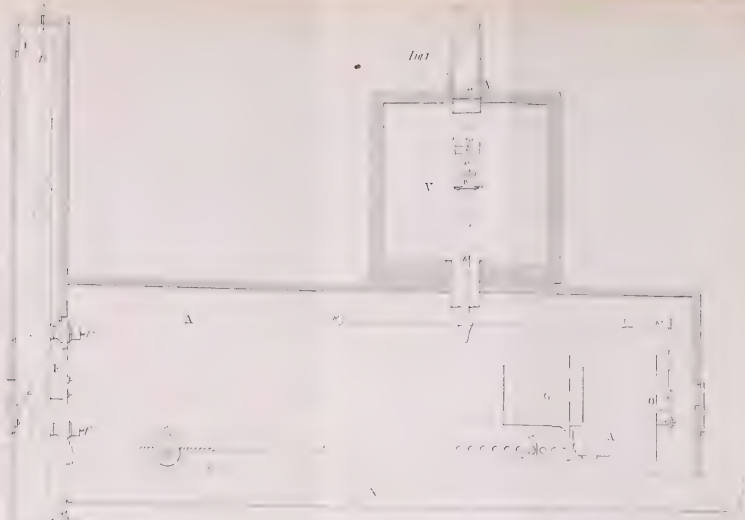


Fig 2



Fig 3

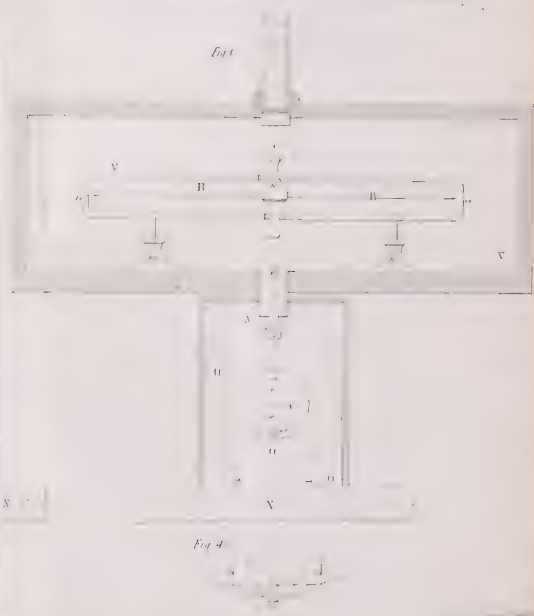
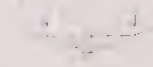
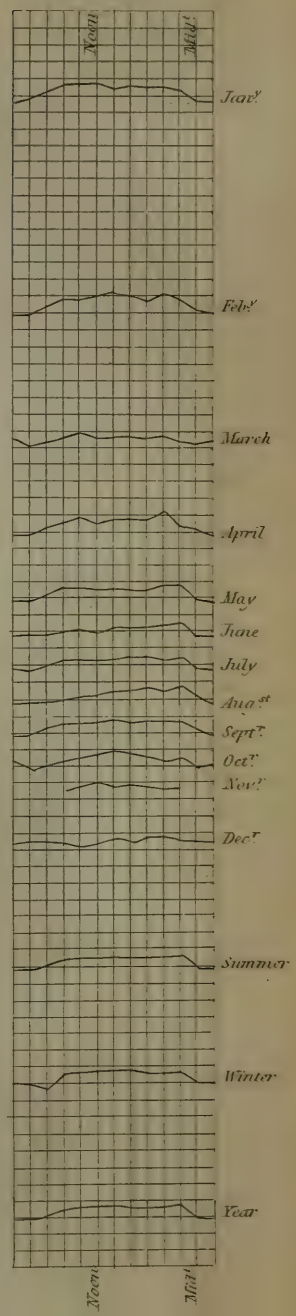
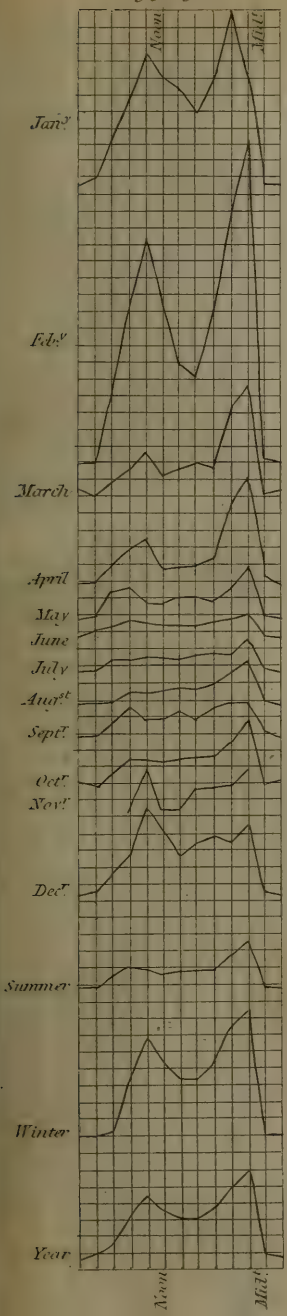


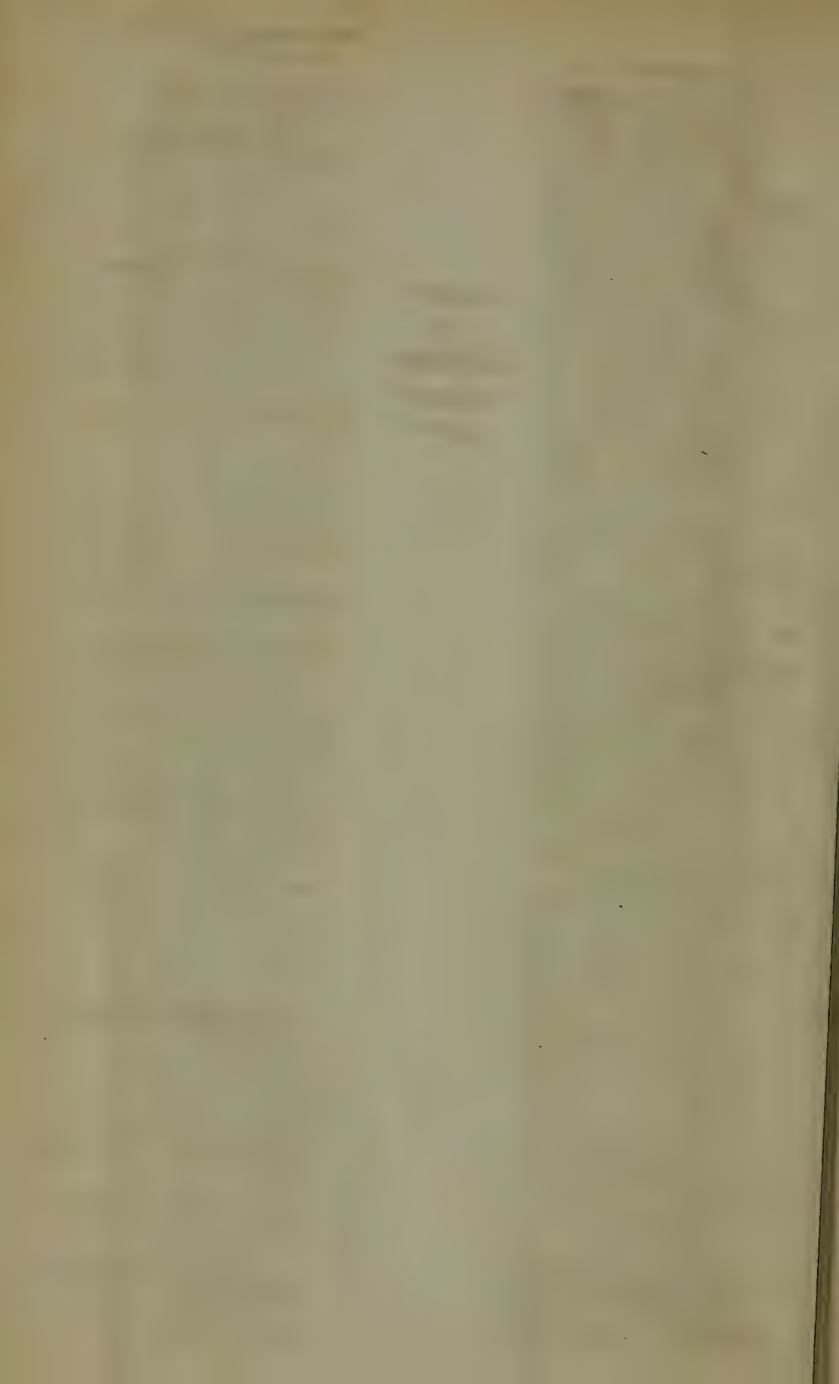
Fig 4



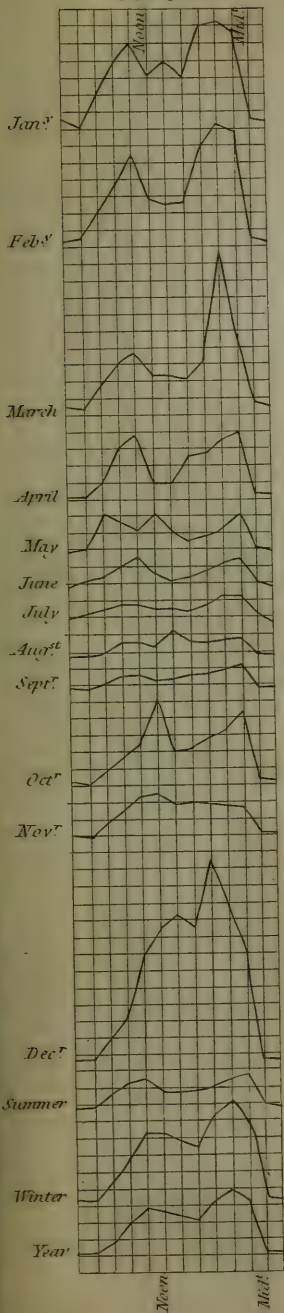
Aggregate

*Curves
of
Electrical
Tension
1845.*

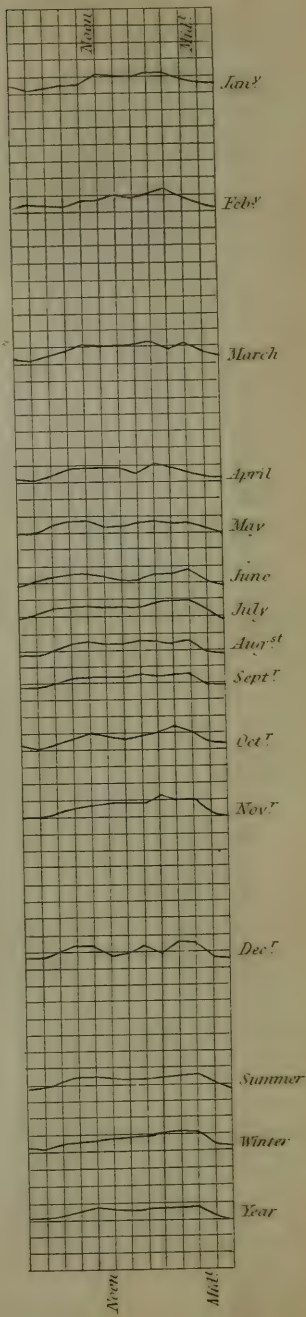




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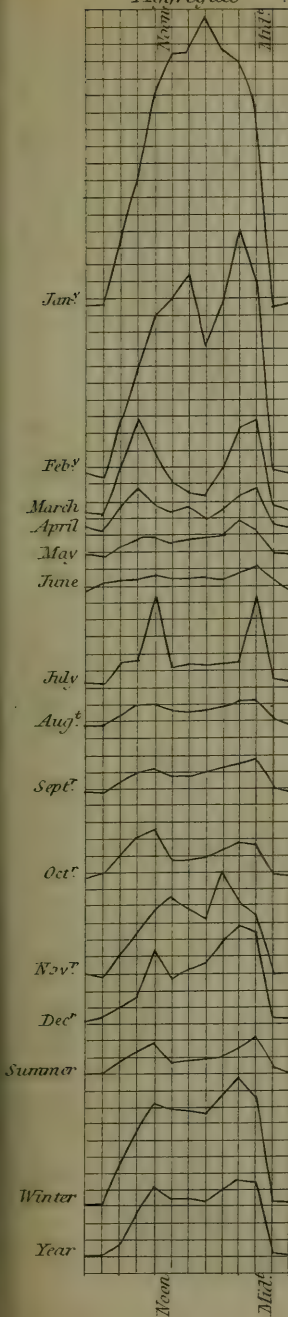
*Curves
of
Electrical
Tension
1846.*



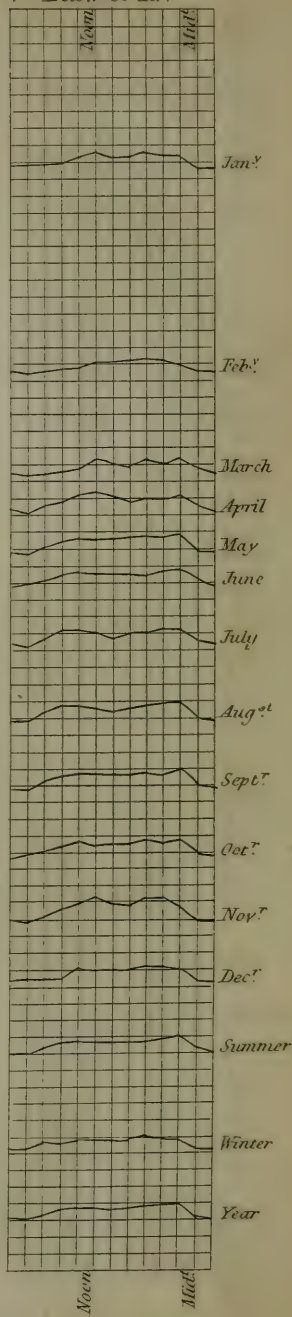


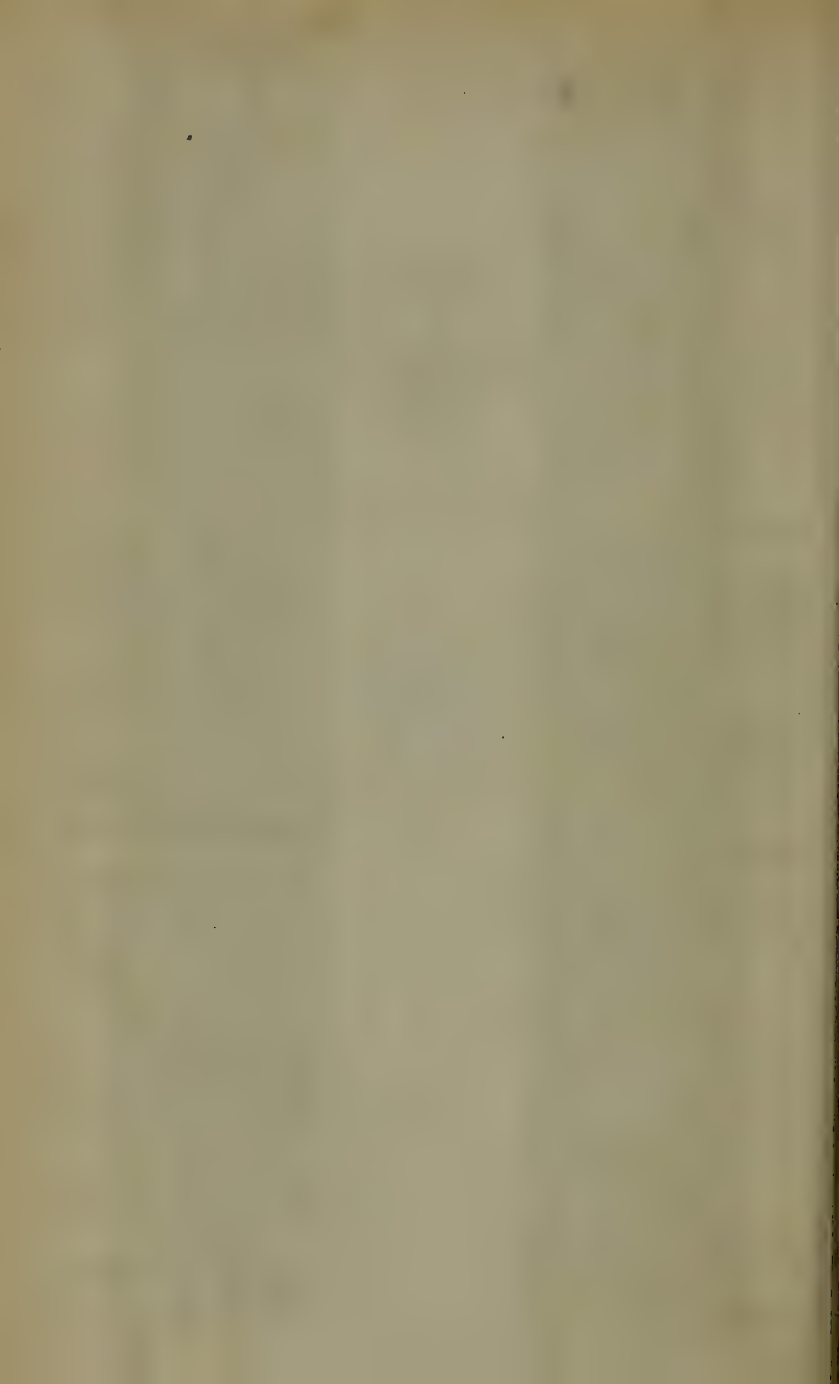
Below 60 div.

Aggregate

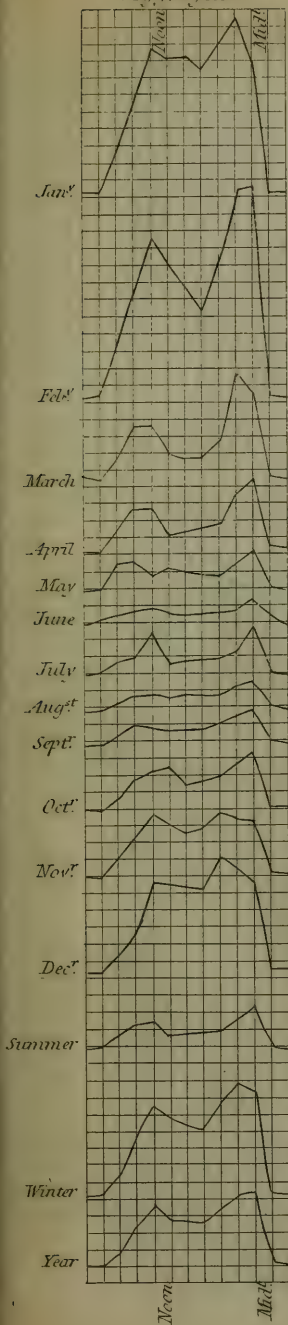


*Curves of
Electrical
Tension
1847.*

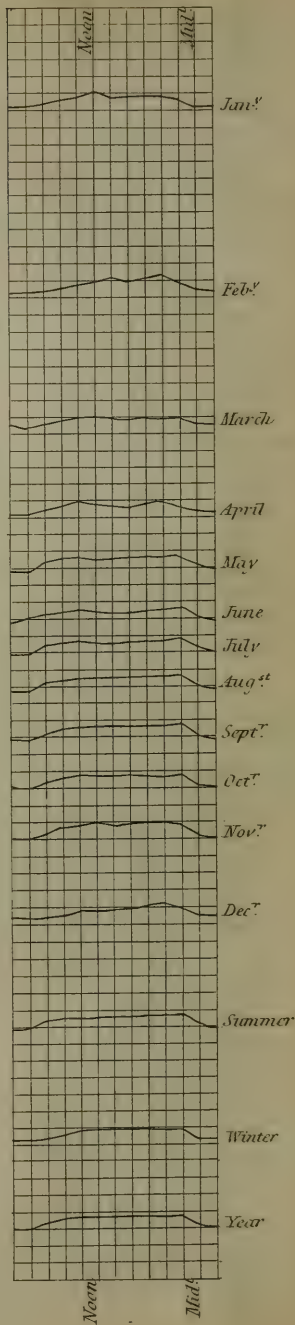




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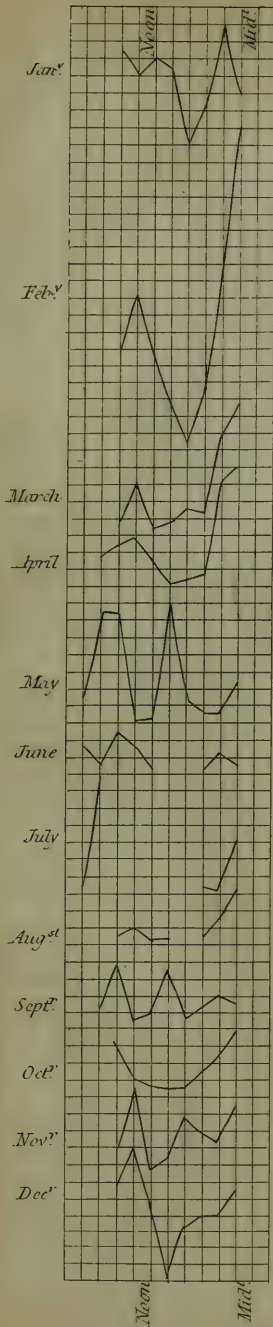


*Mean
Monthly Curves
of
Electrical Tension
from
Observations
in
1845, 1846 & 1847.*

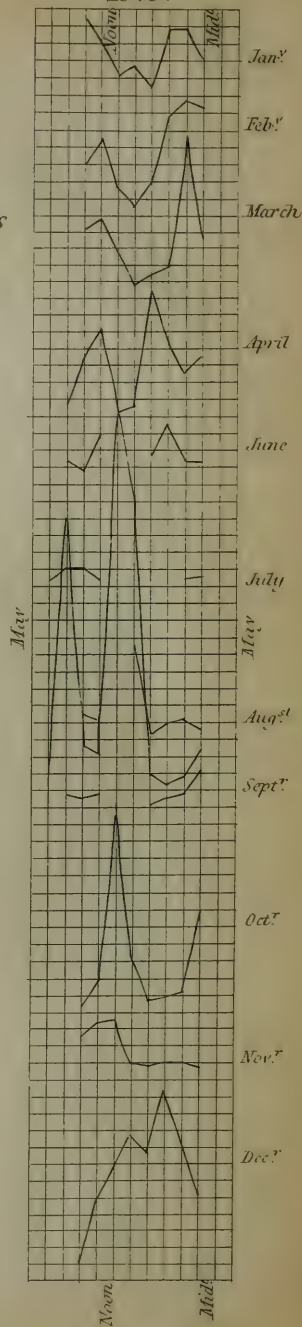


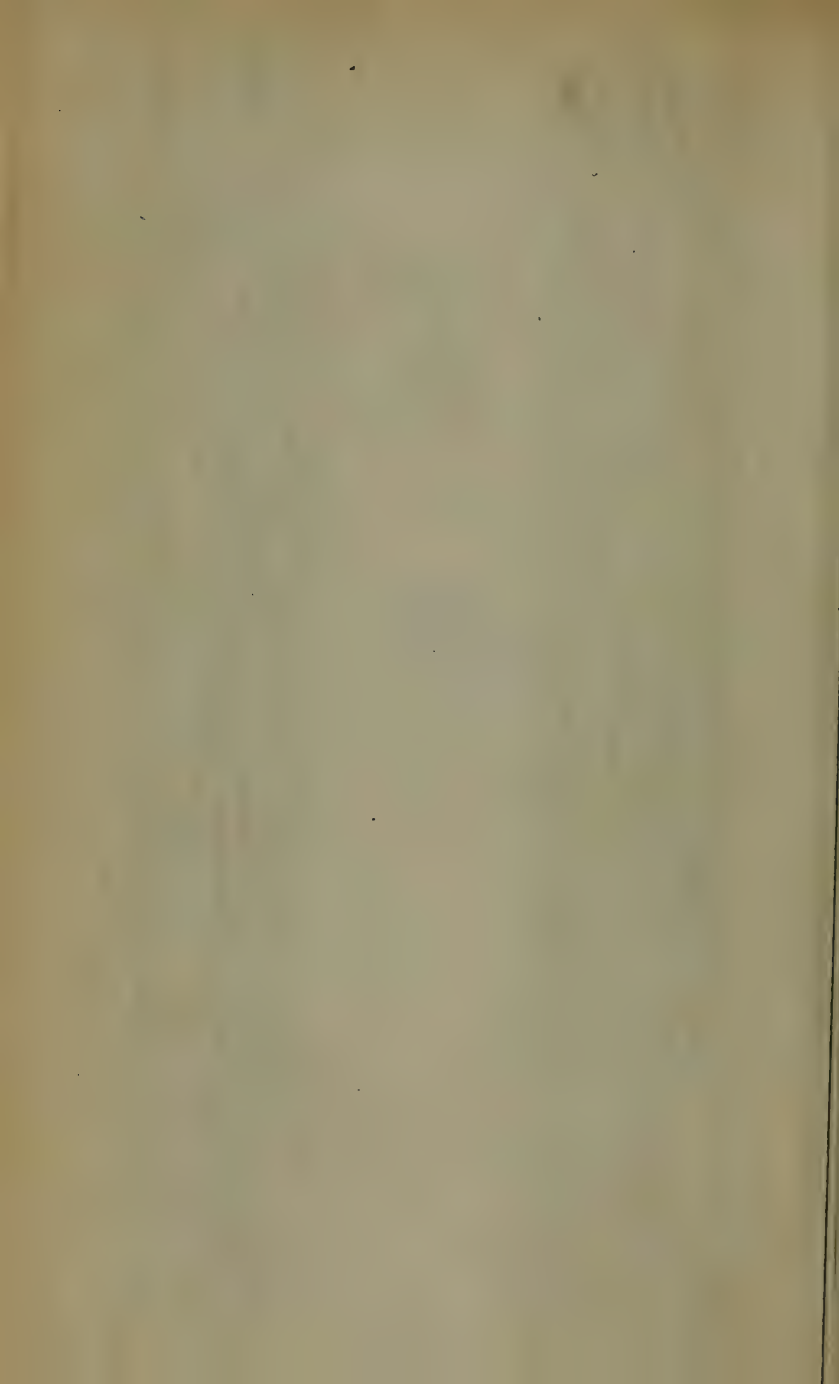


1845.

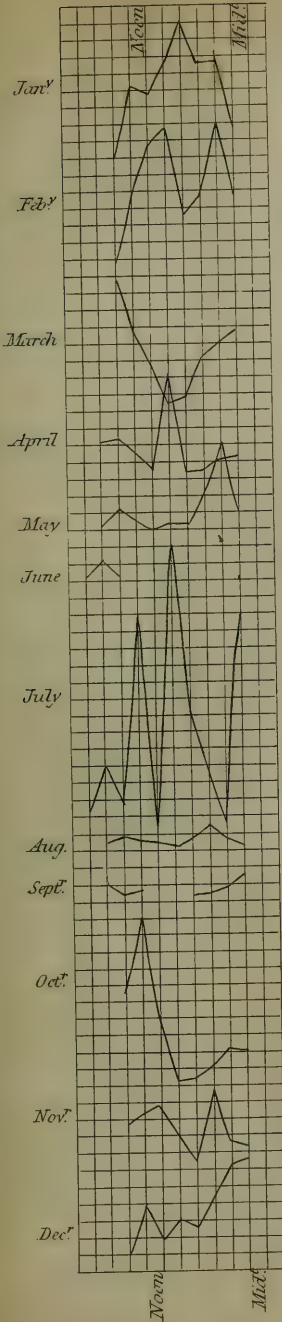


Mean Curves
of
Electrical
Tension.
above 60 div.

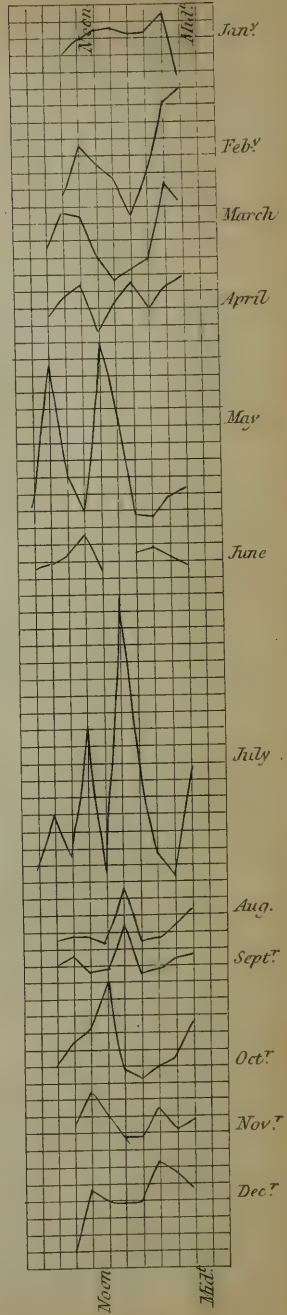




1847.



Mean Curves
of
Electrical
Tension
above 60 div.



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